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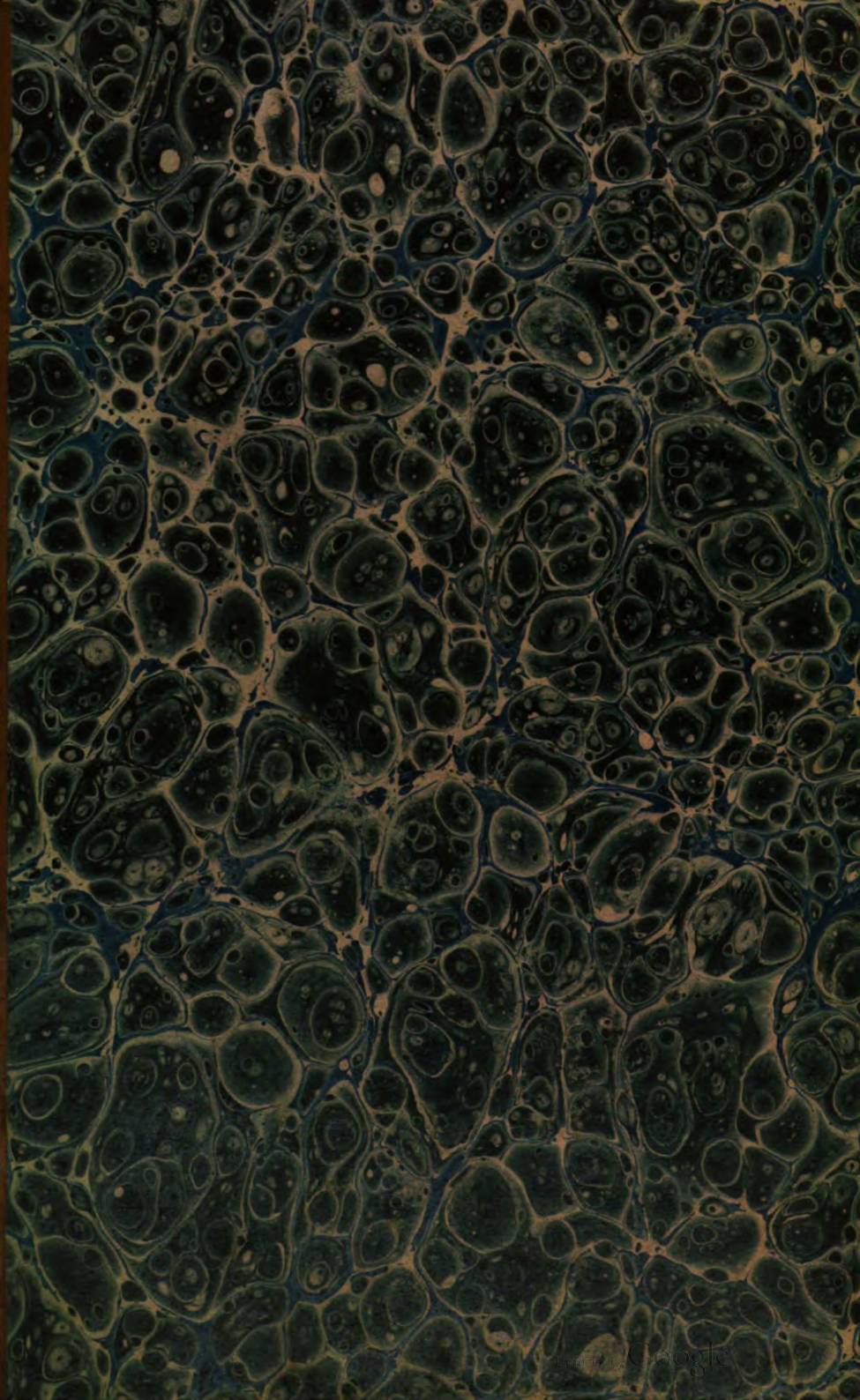
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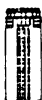
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THE ANNALS
OF
ELECTRICITY,
MAGNETISM, AND CHEMISTRY;
AND

Guardian of Experimental Science.

CONDUCTED BY

WILLIAM STURGEON, M.S.A.

Superintendent of the Royal Victoria Gallery of Practical Science,
Manchester, Late Lecturer on Experimental Philosophy at the
Honourable East India Company's Military Academy, Addis-
combe; &c. &c.

AND ASSISTED BY GENTLEMEN EMINENT IN THESE DEPARTMENTS OF
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THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

JULY, 1840.

I.—*Report of the Committee appointed by the Admiralty to examine the Plans of Lightning Conductors, of W. SNOW HARRIS, Esq. F. R. S. and others. Abridged.*

We beg to observe before detailing the cases which have been brought before us, that we do not consider it to fall within the province of the present report to enter into the general question of the efficacy of conductors in affording protection against the injurious effects of lightning, as this would lead to an investigation of the first principles of electrical action; and the fact of their efficacy may be considered to be established beyond all doubt, by the experience of the last 80 years, and the unanimous opinions of scientific men of all countries.

With reference to the first point to which their Lordships' memorandum directed our attention, viz., "Whether, in cases where ships not having lightning conductors have been struck by lightning, it appears that other ships in company having conductors have not been struck, or have escaped injury?" we beg to adduce the following cases:—

1. In 1815, H. M. S. Norge, was struck by lightning at Jamaica, and lost her maintop-mast and topgallant-mast, whilst the Warrior, 74, which was lying close to her, with her conductor up, received no injury, though the electric fluid was observed absolutely to stream down it. Amongst many other ships which were in Port Royal Harbour at the time, none received any damage but a merchant vessel, which, like the Norge, had no conductor up.

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2. In 1824, H. M. S. Milford, whilst in ordinary in Hamoaze, was struck by lightning, and the foremast and foretop-mast (both very small spars, for the purpose only of making signals) were shattered; she had no conductor up. The Caledonia, of 120 guns, with her lower masts in, and her conductor up, was lying about 80 fathoms distant, and received no injury.

3. In 1824, H.M.S. Phæton was struck by lightning, and the foremast and foretop-mast were totally shivered. The Adventurer was at anchor within a cable's length, with her conductor up, and escaped without any damage, though supposed to have been struck more than once upon that occasion.

4. In 1830, H. M. S. Ætna, when coming to, off Corfu, was struck by lightning, three heavy discharges descending by the conductor, and passing to the water without injuring the spars. The Madagascar and Mosquito, which were in company at the time, and had no conductors up, were repeatedly struck, and received considerable injury.

5. In 1837, the Cochin tank-vessel, in Trincomalee Harbour, was struck by lightning, and her foremast (without a topmast) was shivered, whilst H. M. S. Winchester, at the distance of two cables' length, was uninjured, though the lightning was seen to pass down her conductor.

6. In 1837, the Pelican, sloop-of-war, whilst on the coast of Africa, was struck by lightning on the foremast, and lost her topmast and topgallant-mast; the conductor was not up at the time. The Waterwitch, at two cables' distance, had her conductor up, and escaped injury.

7. In 1838, H.M.S. Ceylon, in Malta Harbour, was struck by lightning, and her pole, foretop-mast, and foremast were shivered; she had no conductor up, and was lying close to the Talavera, Bellerophon, and Dock-yard Sheers, all of which had conductors up at the time, and met with no injury.

These cases have been fully authenticated.

In addition to these instances of the decided protection afforded by conductors, and the disastrous consequences which have arisen from the want of them, we beg to call their Lordships' attention to the case of the New York Packet.

It appears that on her passage to Liverpool, in 1827, this ship was struck by lightning and sustained considerable injury. The conductor was not up at the time; but the weather continuing stormy, it was got out and triced up to the mast head. The ship was a second time struck by a most severe stroke of electricity, which fused the chain, but passed into the water without committing further damage.

It would be easy to multiply instances of the local protec-

tion afforded by metallic bodies accidentally present in ships which have been struck by lightning, as well as cases in which single ships have escaped injury by means of a conductor ; many such have been adduced in evidence before us ; but these cases apply rather to the general question of the advantage of conductors.

Under this head of the Report, however, we may perhaps be allowed to state to their Lordships the result of our inquiries with regard to the common prejudice, that conductors have the power of attracting a flash of lightning, which in their absence would not have fallen on the ship in which they are fitted.

The numerous cases of accidents to ships without conductors, and the comparatively rare occurrence of lightning having been noticed to strike on a conductor, would tend to negative such a supposition ; and it may be observed, that in several instances the electricity has been seen to strike down on the surface of the water at no great distance from a ship fitted with a conductor. This phenomenon occurred in Plymouth Sound, within a moderate distance of the Caledonia, whilst fitted with Mr. Harris's conductors ; and in the instances of the Milford, Cochin, and Ceylon, already mentioned, these ships with very short spars and no conductors, were struck by lightning when within a few hundred feet of ships with considerably higher masts and conductors up ; and in the instance of the Cochin tank-vessel alone was the electric fluid observed to descend on the conductor of the ship which was lying near her (the Winchester), thus affording evidence, either of the little influence exerted by conductors in inducing or attracting an explosive discharge, or of their efficacy in harmlessly and imperceptibly conveying the electricity to the water.

As the objection of the attractive power of conductors has been brought forward by the Surveyor of the Navy, as especially applicable to those of Mr. Harris's principle, it is right to state, in addition to the cases above-mentioned, that with regard to Mr. Harris's, no facts have come under our knowledge which would lead us to coincide in his opinion ; but on the contrary, amongst the several ships fitted on Mr. Harris's plan, which have for many years past been employed in tropical climates, and were exposed, as stated by their commanding officers, to very severe lightning, we have found great difficulty in obtaining direct evidence of their having been struck at all ; and in two or three instances only has the fact been satisfactorily observed, and no case of injury has been recorded.

Professors Faraday and Wheatstone have been consulted on

this point ; and it is their unequivocal opinion that conductors possess no inherent property of attracting or inviting a discharge from a cloud at a distance.

If there be a projecting object, like a mast, within a moderate distance of the point from which the discharge takes place, the electricity will descend by it, whether fitted with a conductor or not, as affording a line of less resistance than it would meet with from the non-conducting property of the air.

"The radius within which it has been considered that a conductor will determine or conduct the electricity is double its own length, provided the discharge takes place within that space, but it has no power to cause the discharge;" on the contrary, "at all times its tendency is to draw off the electricity from the atmosphere, and thereby diminish the liability to an explosion."

In concluding our remarks on this first head of the inquiry, we beg to observe that every search has been made for cases of injury sustained by ships fitted with conductors, and though several statements to that effect have been brought under our notice, not one has been substantiated.

And no instance, so far as we are aware of, has ever occurred of a ship sustaining injury when struck by lightning if the conductor was up to the mast-head, and the continuity uninterrupted to the water.

With reference to the second head of the inquiry, namely :—What conductors have been used in ships, either of the Navy or in those belonging to private merchants? we beg to state, that the conductors which have hitherto been used in the Navy, consist of a copper chain, composed of rods of about two feet in length, and $\cdot 175$, or about one-sixth of an inch in diameter, with an eye at each end. These bars are linked together by rings and the conductor terminates in a rod of the same dimensions, which tapers to a point, and is made with a turn in it near the base, to receive the line to which it is attached throughout its whole length, for stopping to the topgallant backstay when triced to the mast-head.

It should be observed that these conductors are not issued to every ship, but only supplied when demanded, and one only is allowed.

A chain of similar form, composed of either copper or iron, is said to be used occasionally in merchant vessels, but we have had no opportunity of inspecting one.

In the French Navy, a metallic rope composed of mixed metal wire, is attached to the mast-head immediately under the truck, leads down to the top-gallant cross-trees, and thence by the topgallant backstay to the channel, and descends

into the water. A copper spindle of about three feet in length, tapering from an inch to a point, is screwed into the mast-head, nine inches of the upper end being hardened and gilded.

This description was obtained by Mr. Rice, foreman of shipwrights, at Chatham Dockyard, from the officers of the French frigate *Calypso*, in 1832, when under repair. A piece of it was produced for our inspection, composed of three strands of eight wires each, and measuring one-eighth and a half inch in circumference.

Mr. Harris's conductors, which have been fitted for trial in the ships named below*, are composed of two plates of copper rivetted together, so as to form an elastic and continuous line of metal; the inner plate being one-sixteenth and the outer one-eighth of an inch in thickness, their breadth varying according to the class of the width of the plates which have hitherto been used, as they are considered to be unnecessarily large, and the subject will be discussed in the sequel.

These plates are inserted in dovetailed grooves, in the after part of the masts, and extend from the truck to the keelson; a copper plate of the same dimensions is led over the caps, and the continuity is preserved at all times by a tumbler on the caps, consisting of a short copper bar with a hinge at the base, by which it leans against the conductor of the topmast, whether fidded or housed; and their lordships will perceive by reference to the drawings which accompany the Appendix, that a stop is placed on the exterior by which the tumbler is prevented from falling backwards.

Copper plates of equal dimensions to those on the lower masts are placed under the heels and steps of the masts, and are thence led along the keelson in contact with the copper fastenings.

In order to insure connexion with the copper sheathing, bolts are driven transversely through the keel, so as to meet those passing down from the keelson.

Copper plates are likewise led along the underside of the beams of the lower and orlop decks to the principal copper fastenings, and ultimately terminate in the sheathing, thereby combining all the chief masses of metal in the hull and spars of a ship with the conductors, and affording by means of its ultimate connexion with the copper sheathing a vast surface in contact with the water for the dispersion of the electricity.

With reference to the third head of the inquiry, namely: What the objections are to the conductors now in use? we beg to state, that the chief objections of a practical nature which

* *Actæon, Asia, Beagle, Belvidera, Blanche, Caledonia, Dryad, Druid, Forte, Revenge, Saphire, St. Vincent, Spartiate.*

have been urged against the common chain conductors, are that not being fixtures, they are seldom ready when required, are kept packed in a box, and usually stowed away in the store-rooms, and when thunder squalls arise suddenly and unexpectedly, as frequently occurs, especially in the tropics, the damage is done to the ship before they can be got out and triced up. At all times there is danger in tricing them up, as it is usually done when lightning is anticipated.

In 1834, on board the *Thunderer*, the men had not left the conductor five seconds when the lightning descended with extreme violence; and in one instance, on board a vessel in the mouth of the Mississippi, several men were struck dead at the moment of hoisting one up.

In dark nights the difficulty of tricing them up properly is greatly enhanced, and in heavy weather, when much needed, it has been found impracticable to get them up at all.

Their construction is very slight, and the rings not being welded together, a trifling strain breaks them.

In the event of a topmast or topgallant-mast being carried away the conductor is likely to be lost, and at any rate the ship is unprotected until it can be got in, and triced up to another mast. This case occurred to the *Jupiter*.

As conductors, their capacity is not sufficient for the safe transmission of heavy discharges of electricity, and in several instances the metal has been fused or disjoined. This occurred to the *Dublin* and *Ætna*.

In short, we cannot but regard them as a temporary and inadequate expedient.

By not being permanently fixed, the security of the ship is left to the opinion of the commanding officer as to their utility at all, or necessity at the moment.

They are not calculated to be applied in all weathers, are subject to all the casualties to which the ship's rigging is exposed, and liable to lead to serious accidents by the end being brought in board, the continuity interrupted, or the end lifted out of the water.

4. What the advantages or disadvantages of Mr. Snow Harris's conductors, as compared with others?

The advantages to be derived from the adoption of Mr. Harris's plan are, the removal at once of all the objections and liabilities to which the common chain conductor is exposed.

A continuous line of metal from the truck to the water is permanently fixed, and if it be found necessary to strike any of the masts, or one or more be carried away, a safe conductor will still remain. By its connexion with the detached masses of metal used in the fastenings of the hull, and its final junction

with the copper sheathing, the important advantages of great electrical capacity are obtained, and of ready means under all circumstances, for the rapid diffusion of the electricity over a vast surface of metal in contact with the water.

Professor Faraday stated to the committee, that it was his opinion that "Mr. Harris's conductors met every case that he could possibly conceive to occur, and offered no one disadvantage or objection whatever;" and Professor Wheatstone stated, that, 'he could see no objection whatever to Mr. Harris's conductors in a scientific point of view."

A committee of the Royal Society, appointed in 1823, to consider the merits of these conductors, as well as Dr. Wollaston and Sir Humphry Davy, have stated their approval of the principle of Mr. Harris's plan.

With reference to the disadvantages of Mr. Harris's conductors, we beg to state that all the objections which have been brought against them have, to our minds, been sufficiently removed by the evidence adduced before us; it will be proper, however, to state these objections to their Lordships, with the facts and opinions which have influenced our conclusions.

The objections may be divided under the following heads:—

1. Those of a scientific nature, involving principles of electrical action.

2. Those of a practical description, as tending to injure and weaken the spars.

3. The indirect objection on account of expense.

1st. Theoretical objections.

First. That Mr. Harris's conductors attract the lightning.

This applies equally to all conductors, and has been already refuted.

Secondly. That danger arises from the "lateral explosion."

Mr. Martyn Roberts has objected to the conductors being led through the body of the ship, on account of the dangers of lateral explosion, which he considers might set fire to the ship, or ignite the magazine. His hypothesis is, "that when a discharge of electricity passes along a conductor, visible sparks would be thrown off from the sides of the conductor to any metallic or other conducting body within a moderate distance, and be capable of igniting inflammable substances." "And that such lateral discharges would be in proportion to the capacity and proximity of the secondary conductors, with reference to the volume of electricity passing down the primary conductor."

Professor Faraday stated, that "he was not aware of any phenomenon called lateral discharge, which was not a diversion or division of the primary current, and that all liabilities to

a diversion of the main charge would decrease in proportion to the capability and goodness of the primary conductor; that in proportion as the number of the metal bolts are connected with the conductor would the probability of a lateral diversion diminish."

"It was his opinion that a lateral discharge could not be obtained from Mr. Harris's conductor, provided the continuity were not interrupted; and from the increased dimensions of the plates at the lower extremity, and the complete mode of connexion with the fastenings of the hull, the electricity would be so rapidly diffused that he doubted whether, with any intentional contrivance, the magazine could be ignited from the sides of the conductor. He could not but appeal to the evidence of experience to prove the efficacy and safety of Mr. Harris's plan; ships fitted with his conductors had been exposed to severe lightning, and the electricity had been known to descend by them with perfect security to every thing on board; nor was there, so far as he could learn, any instance on record of lateral explosion."

Professor Wheatstone stated, with regard to lateral explosion, that "all the cases with which he was acquainted were those of a partial diversion of the main current where the conductor was not of sufficient capacity of conduction, in which case a portion of the electricity distributes itself to any tolerably good conductor within a moderate distance.

"This, however, had only been known to occur from a very small wire, and from a conductor of the dimensions proposed by Mr. Harris, would be impossible, with such atmospheric discharges as we are acquainted with.

"The liability to such lateral diversion would be diminished in proportion to the means of diffusion; and considering the mode proposed by Mr. Harris for connecting the principal fastenings, &c., of the hull with the conductors, no danger need be anticipated from leading the electricity through the body of the ship or within a few feet of the magazine so long as the continuity was maintained."

Notwithstanding this evidence, Mr. Martyn Roberts subsequently communicated to the committee that he had made further experiments on a larger scale, which favoured his idea of the danger to be anticipated from lateral explosion."

Professor Wheatstone was in consequence requested to attend to receive from Mr. Roberts himself an explanation of the experiments which he had instituted; and we beg to subjoin the further opinion of Professor Wheatstone on the subject, which he communicated in writing.

"When the known conditions of a good lightning conduc-

tor are fulfilled, it is physically impossible that it should occasion the least accident to the building or ship to which it is attached. When injury does occur to a ship provided with one, it is because this conductor is not sufficient to carry off the whole of the discharge; the ship is then only partially protected—damage is done; but this damage must be in all cases immeasurably below what would have been produced by the whole discharge, had it not found any conductor to transmit it to the water. The danger to be apprehended from the division of the discharge may be reduced to almost nothing by increasing the dimensions and conducting power of the bars or plates which transmit the electricity, and by keeping good conductors, not connected with it, out of its immediate vicinity.

“It has been proved beyond doubt that electricity follows the best conducting path which is open to it; and that when it finds a metallic road sufficient to conduct it completely, it never flies to surrounding bodies greatly inferior in conducting power.

“The experiments of M. de Romas, made in France, with the electrical kite, immediately after Franklin's first attempt, might satisfy the most timid in this respect. ‘Imagine,’ writes he to the Abbe Nollet, ‘that you see sheets of fire, nine or ten feet long and an inch broad, which made as much or more noise than the reports of a pistol. In less than an hour I had certainly 30 sheets of these dimensions, without counting a thousand others of seven feet and under. But what gives me the greatest satisfaction in this new spectacle is, that the largest sheets were spontaneous, and notwithstanding the abundance of fire which formed them, they constantly fell on the nearest conducting body. This constancy gave me so much security that I did not fear to excite this fire with my discharger, even when the storm was violent; and when the glass branches of this instrument were only two feet long, I conducted wherever I pleased, without feeling the smallest shock in my hand, sheets of fire six or seven feet long, with the same facility as those of only six or seven inches.

“The wire of the kite was insulated, and the sparks drawn by a metallic conductor held in the hand by means of an insulating handle, and communicating with the ground by a chain. The human body is known not to be one of the worst conductors; yet because it was two feet further than a far more perfect one, it received none of the discharge, even though the conducting path was an interrupted one.

“The phenomenon to which the name of lateral explosion has been generally given was the first observed by Henly, Vol. V.—No. 25, *June*, 1840. B

more than half a century ago, and has been subsequently experimented upon by Priestly, Cavallo, and more recently by Biot.

"I conceive it has no application to lightning conductors, but as it has been brought forward as an objection to Mr. Harris's plan by Mr. Roberts, it may be necessary to say a few words respecting its real nature.

"It takes place during the discharge of an electric battery, that is at the moment of the union of the positive and negative electricities accumulated on the opposite coatings of the jars; no part of these accumulated electricities has anything to do with the effect, which arises solely from the induction of that small portion of electricity which remains free on one of the surfaces of the battery or the conducting bodies attached thereto.

"This is the explanation of the phenomenon given by the best authorities, and, as Biot observes, theory and experiment unite in demonstrating to us that it is incomparably less than the direct discharge.

"Even, therefore, were lightning conductors liable to this lateral discharge, it would be easy to prevent any material damage arising from this cause; but after attentively considering the subject, and Mr. Roberts's objections, I am still of opinion that, in the case of lightning conductors, the lateral discharges that sometimes occur and produce mischief, arise solely from the insufficiency of the conductor to carry off the whole of the electric fluid which enters, as I have above stated, and the remedy to which is obvious."

The evidence of the officers who had served in H. M. ships fitted on Mr. Harris's plan, and had witnessed the effects of lightning descending by the conductors, as well as the absence of any case, so far as we can learn, of lateral explosion, even from the common chain conductor of such inferior capacity, so fully bear out the opinions of Professors Faraday and Wheatstone, that we do not hesitate to state our entire conviction of the futility of the objection on account of "lateral discharge."

Thirdly. That Mr. Harris's conductors do not afford a continuous line of solid metal. This objection will embrace the ninth question in their lordship's instructions, namely, "Whether the continuity can be preserved in all probable circumstances, and whether the danger is not increased in case of interruption of the conductor, or of its being of inadequate dimensions?"

On this point again we beg to quote the opinions of Professors Faraday and Wheatstone.

Professor Faraday stated, that "the conducting power of the plates would be but little diminished by the continuous solidity of the metal being interrupted, so long as the portions of the conductor remained in contact; that even supposing the tumblers on the caps were, through accident, to be open to the extent of half an inch or an inch, no injury would be caused to the surrounding woodwork by the electricity leaping from one point to the other; that rope, or any substance of small conducting capacity, if placed between the two points, would perhaps be destroyed, but the probability appeared so small of any accident occurring whereby the continuity could be interrupted, that he should not hesitate to say there would be no objection to Mr. Harris's plan on that score."

Professor Wheatstone stated, "he was of opinion that if the copper plates of which the conductors were composed were in mechanical contact, there would be no danger of an explosive discharge along the line of junction; and that their capability for carrying off the electricity would be so little diminished by a slight interruption of the continuous solidity of the metal, that there could be no objection to them on the ground of being formed of separate pieces of copper."

"That the continuity of the conductors appeared to be sufficiently provided for by the tumblers on the caps, and that no danger need be anticipated supposing they were opened by accident to the extent of an inch or two, as the electricity would pass from one point to the other without damaging the contiguous woodwork."

Their lordships will perceive by reference to the description and drawings of Mr. Harris's plan that every means have been adopted to ensure the preservation of the continuity under all possible circumstances, and in no case is an interruption of any consequence likely to occur.

Fourthly. The danger of accidents to men in contact with the conductors at the moment of the electricity descending.

No instance of any accident of the sort has been known to occur.

On board H. M. S. Beagle, the lightning was seen to strike the conductor, and though it passed within eight inches of the purser's head, who was asleep in his cabin at the time, he experienced no ill effects, beyond being woke by the general concussion.

Professor Faraday stated, that he believed a man would receive no injury if he were leaning against Mr. Harris's conductor when the electricity descended, and that any opinion to the contrary must be only assumption.

2. Objections of a practical description, as injuring and weakening the spars.

The surveyor of the navy and Mr. Edye stated that they were of opinion that Mr. Harris's conductors injure the spars.

Frist. That the nails by which they are fixed split the spars; and when the masts are strained by carrying sail, the wet might get into the splits on the weather side, and cause injury.

Secondly. That the conductors weaken the spars.

In support of the opinion that the nails split the spars, the surveyor of the navy considered he was borne out by the report of survey on the *Caledonia's* spars, from which Mr. Harris's conductors had been stripped.

The officers of Plymouth Dockyard state, in their report of the 15th June, 1839, that "if the conductors had been allowed to remain in the spars, there would not have been any objection to their re-issue to the ship or ships to which they belonged."

"That if the conductors had not been removed from the topgallant-masts and flying jib-boom, 'the rents occasioned by the nails would not have been apparent,' and the necessity for reducing the spars on account of the grooves made for the reception of the conductors would not have existed." "In several instances the injury done to the smaller spars may be attributable to the great number of nails used for fastening the conductors, and which rendered the small spars of the *Caledonia* unserviceable."

We beg to observe, however, in this instance, that nails are stated to have been used of two inches and a half in length to fix the copper plates, of only three-sixteenths of an inch, on a spar of six inches and a half in diameter.

The foreman of the Mast Department at Plymouth states, that "none of the spars of the *Spartiate* and *Forte* when returned into store were rendered unserviceable from the conductors, and in every instance the plates were as securely fixed as when first fitted in; that no injury from nail-holes would ever render a re-conversion of a spar necessary; and that they would never be rendered inapplicable for other or inferior purposes, if the conductors were kept in."

The officers of the Portsmouth Dockyard state that "they are not aware that any injury to the masts or spars was attributable to the application of Mr. Harris's conductors." Captain Fitzroy stated, that "in *H. M. S. Beagle*, when under his command, he had never found the spars split by the nails, and did not consider that they were likely to be weakened or injured by them, as the nails were flattened at the point, and passed between instead of cutting the fibres of

the wood; but allowing such to be the case, no wet could ever penetrate if the masts were kept properly greased."

Secondly. That the conductors weakened the spars, (which embraces the sixth question of their lordships' instructions, namely, "Whether the conductors of Mr. Snow Harris can be so fitted as not to weaken the spars in which they are placed?")

Captain Fitzroy stated, that "the copper plates appeared to strengthen rather than weaken the spars. In so small a vessel as a 10-gun brig, the *Beagle*, the spars were found to be improved rather than injured, and though exposed for five years to continued service, the same spars remained at the present moment in the *Beagle*, with the exception of the top-gallant-masts."

Commodore Pell stated, in proof that Mr. Harris's conductors were not injurious to the spars, that after four years' service the *Forte* was paid off with the same masts, top-masts, topgallant-masts and royal-masts.

Captain the Hon. F. Grey, Commander Turner, Captain the Hon. W. Wellesley, and Commander Norcott, who had served in ships fitted with Mr. Harris's plan, were also equally of opinion that the introduction of the copper plates tended rather to strengthen the spars. Experiments were made to ascertain the point in 1831, in Portsmouth Dock-yard, under the superintendence of Mr. Harris and Mr. Rice, in the presence of several distinguished officers, &c., by which it will be seen that a spar (a jib-boom) was undoubtedly strengthened by the application of the plates, and in certain positions increased in stability upwards of a sixth; thus confirming the opinions of the officers above mentioned, who had tested the fact by experience in actual service.

Thirdly. Objections on the score of expense.

With reference to the fifth point of their lordships' instructions, namely, "What the comparative expense is of different descriptions of lightning conductors?"

The accompanying Table shows the cost of Mr. Harris's, and the common chain conductor for each class of H. M. ships.*

* In this Table the expense of the common conductors is omitted, but we have added this information from another table which we find in the Appendix.—ED. M. M.

Class of Ships.	Expense of fitting each Ship with Harris's Conductors.			Expense of common Conductors.			
	No. of Guns.	£.	s.	d.	£.	s.	d.
120	365	17	8	3	2	8	$\frac{1}{2}$
84	350	15	7	3	1	3	
74	317	18	6	2	16	10	$\frac{1}{2}$
50	286	15	10	2	13	11	$\frac{1}{2}$
46	236	1	7	2	8	1	$\frac{1}{2}$
28	161	18	11	1	19	8	
18	119	7	2	1	19	8	
10	102	12	7	1	15	0	

From this account it is obvious that the adoption of Mr. Harris's plan would be accompanied with a very considerable increase of expense, but we propose to show in the sequel by what means certain reduction in their cost may be effected.

We now beg to state, with reference to the tenth head of the inquiry, namely, "Whether any other mode of fixing lightning conductors does not possess the same or greater advantages than Mr. Harris's?" That Mr. Martyn Roberts submitted to us a proposition for avoiding the dangers he considered likely to arise on the adoption of Mr. Harris's plan, from the alleged lateral explosion, by means of a rope composed of annealed copper wire, to be led from the truck down the afterpart of the masts to the lower mast-head, and thence as a backstay to the copper sheathing, to which it is to be soldered or brazed. This plan differed only from the conductor used in the French navy in its mode of application.

Mr. Edye also submitted a plan for obviating the supposed objections to Mr. Harris's conductors from their passing through the hull of the vessel, consisting of Mr. Harris's copper plates as far as the head of the topgallant-mast, (or if necessary to the top-mast head), and wire-rope back-stay on each side down to the copper sheathing.

Before entering into the merits of these two plans of very similar construction, we cannot but remark on the circumstance that the chief objections urged by Mr. Edye and the surveyor of the navy against Mr. Harris's conductors equally apply to Mr. Edye's proposition, namely, the injury to the small spars from the nails by which the copper plates are fixed, and the tendency of the conductors to attract lightning.

Mr. Edye, in his evidence, states, "he considers Mr. Harris's plan would decidedly weaken the spars, and the nails unquestionably injure by causing splits and admitting the wet, as

was found in the case of the *Caledonia*;" while his own proposition is to apply Mr. Harris's plates to the royal-masts and topgallant-masts if thought necessary, these masts being the most liable to the injury he so unquestionably states must, in his opinion, ensue.

Professors Faraday and Wheatstone were shown the drawings and descriptions of these conductors.

The former observed with regard to Mr. Edye's, that "there was no doubt that if they could be kept in their places under all circumstances, and the rope was of sufficient capacity to carry off the electricity, they would be efficacious; but in his opinion, their liability to derangement was far greater, their capacity less; nor were they in any one point equal to Mr. Harris's; and he should greatly prefer the latter."

Professor Wheatstone stated, that "Mr. Edye's plan of a conductor appeared to him to be liable to all the casualties to which the common chain conductor was exposed. If it could always be kept permanent, and the wire ropes were of sufficient capacity, there would be no doubt they could lead off the electricity; but their liability to accidents was an insuperable objection.

"Mr. Roberts' plan appeared very similar to the metallic rope conductors used in the French navy, and was objectionable on the same grounds as Mr. Edye's."

We entirely concur in the opinions of these gentlemen, and beg to observe that both Mr. Roberts's and Mr. Edye's plans appear to us to be equally subject to all the liabilities to which the rigging of a ship is exposed in common with the chain conductor; in the event of a topmast being carried away, the ship is left unprotected, and thus, in the hour of danger, they are liable to become useless.

The weight of the wire ropes in Mr. Edye's plan would be a great objection, especially when it is considered that the whole of this weight rests on the head of the topgallant-mast alone.

A wire rope, of three-fourths of an inch in diameter, from the topgallant-mast head (on each side) of a first rate and an 18-gun brig, as compared with the weight of their hempen backstays, would be as follows, namely:

	Hemp.	Wire.
First-rate: topgallant backstay...	246lbs.	357lbs.
18-gun brig.....	92	268

And as it is necessary to provide conductors for each mast-head, and their capacity must be the same, whether in a first-rate or sloop-of-war, the comparative excess of weight would be still greater in the fore and mizen masts.

In carrying sail, especially in dry weather, when the rigging is slack, the metal not being affected in the same degree as the hempen ropes, the strain would probably be so great on the conductor, as to carry away the topgallant-mast, or the wire backstay.

It has been proposed to place a globe of glass on the mast-head in lieu of a conductor, on the hypothesis that, from the non-conducting property of glass, it would serve as a repellant to lightning; but Professor Faraday considered "it would not be a preventive, but would rather tend to increase the liability to an explosive and to a more violent rather than to a silent discharge, and would therefore increase the danger."

Professor Wheatstone was of opinion that "such a proposition was an absurd notion, and would be dangerous in the extreme, inducing, in many instances, an explosive discharge, where a conductor might have silently drawn off the electricity."

After maturely considering the several points now discussed, and the evidence, both practical and theoretical, which has been submitted to us, we are unanimously of opinion, that of all the plans of conductors which we have had under our consideration, that proposed by Mr. Harris affords the best means of preventing the injurious effects of lightning.

We now propose to show by what means the expense of fitting Mr. Harris's conductors may be reduced.

In considering this question, it will be necessary to divide it into three separate heads, namely:

First, Dimensions of the plates of the conductors.

Secondly, Abbreviation of the conductors.

And, Thirdly, The number required in each ship.

First, in order to ascertain the feasibility and safety of reducing the size of the copper plates originally proposed by Mr. Harris, it is necessary to enter into the question of the requisite dimensions of metallic rods to insure protection against lightning.

The capability or power of a metal rod for the safe transmission of electricity is in direct proportion to the area of section or its metallic contents.

A copper rod, of half an inch in diameter, has never been known to be fused or heated red hot by an atmospheric discharge of electricity, and thus a standard of sufficiency is afforded with which all conductors may be compared.

On consideration of this fact, and the rare occurrence of the common chain of one-sixth of an inch being fused, we were led to conclude that Mr. Harris's plates were larger, especially in the lower masts, than experience seemed to require for the safe conduction of electrical discharges, and as their dimen-

sions varied in the different classes of ships, and it was apparent that whatever was requisite for one was necessary for all, without reference to the size of the ship, we desired Mr. Rice to prepare a table of the comparative dimensions of Mr. Harris's conductors on the scale originally fixed by him and that proposed by us, and Mr. Harris having expressed his entire acquiescence in the reductions, we beg to recommend the following scale of dimensions for the copper plates for the masts and spars of ships of all classes in the event of these conductors being used in future in H. M. Navy, namely:—

	Inches in width.
Lower masts and bowsprits	4
Topmasts and jib-booms	3
Topgallant-masts, and flying jib-booms	2½
To taper from hounds to truck from	2 to 1½

The plates remaining of the same thickness in all, viz. $\frac{1}{16}$ ths of an inch.

These reductions will effect a commensurate diminution in the expense, and the following account shews the cost of fitting each class of H. M. ships on the scale proposed; viz.

	Total Expense of the Conductors.	Value of copper as "old copper," when no longer serviceable as conductors.	Actual Cost to the Crown.
	£.	£.	£.
First-rates	258	133	125
Second-rates ...	246	127	119
Third-rates	230	119	111
Fourth-rates ...	214	110	104
Fifth-rates	192	99	93
Sixth-rates	151	85	66
Sloops	98	47	51
Brigs	87	43	44

In order to remove any doubt as to the security with which these reductions may be effected, we beg to quote the following opinions.

Professor Faraday stated, that "he had no doubt the reductions of the copper plates proposed by the committee could be effected with entire security."

Professor Wheatstone stated, that "in the Report of the Committee of the Academy of Sciences of Paris, appointed to investigate the utility of lightning conductors, it is mentioned that there is no instance on record of an iron rod of half an inch in diameter being fused or made red-hot by a flash of lightning; and considering that the capacity of copper for the conduction of electricity was from six to eight times greater than that of iron, and that the area of the section of Mr. Harris's conductors at the mast head was $\cdot4688$, of a square inch, and in the lower masts $\cdot7500$, whilst that of an half-inch rod was $\cdot1970$, he felt convinced that they were perfectly safe."

Secondly. Abbreviation of the conductors.

It appeared to us to be a question of great importance to consider, whether the time and expense of docking ships for the purpose of drawing the copper bolts to effect the metallic continuity with the keelson bolts and the copper sheathing might not be dispensed with in cases of emergency, when ships were required to be fitted out with expedition, and at the same time that they might possess the advantages of these permanent conductors. We therefore submitted to Professors Faraday and Wheatstone, whether sufficient security would be afforded against lightning if the conductors were led down no farther than the orlop-deck in line-of-battle ships and frigates, and to the lower deck in sloops and brigs, and the metallic connexion with the copper sheathing maintained alone by the transverse copper bands leading under the beams.

We beg to subjoin their opinions.

Professor Faraday stated, that "he is of opinion there would be no objection to cutting off the lower portion of the conductors, say from the lowest deck, provided that four, or even only three of the transverse copper bands leading under the beams to the copper sheathing remained, as these would afford ample means for the dispersion of the electricity.

"There would be no danger whatever from the electric discharge being thus deflected at right angles from the perpendicular line, as it would always take the line of least resistance, and is totally independent of momentum."

Professor Wheatstone stated, that "if, for the sake of economy, the conductors were carried no further than the lowest deck instead of to the keelson, he was of opinion that the transverse copper plates under the beams, if connected with the copper sheathing by conductors of sufficient capacity, would afford ample means for carrying off the electricity, and that there could be no objection to such an alteration, provided always the continuity could be equally well maintained."

The reduction in the expense of fitting the conductors on

this plan, as compared with the former account, is shewn in the following estimate, viz, :—

	To the Lower Deck. Total Cost.	To the Keelson. Total Cost.
	£.	£.
First-rates	229	258
Second-rates	218	246
Third-rates	206	230
Fourth-rates	191	214
Fifth-rates	171	192
Sixth-rates	136	151
Sloops	88	98
Brigs	79	87

By this plan of abbreviating the conductors, a reduction of about one-seventh of the expense of fitting ships would be effected ; and as there appears to be no objection on scientific grounds, it may be resorted to as a safe expedient in cases of emergency. But we still would beg to recommend that the copper plates be carried down to the keelson, in all ships which may be built or docked in future.

Thirdly. The number of conductors required in each ship.

The question of the necessity of having conductors fitted to each mast and the bowsprit depends upon the confidence to be placed in the supposition that a conductor protects within a radius of double its own length, a supposition which may be considered to stand in need of confirmation by further experience.

In stormy weather, when accidents are most likely to occur, a ship may carry away a mast, and if not fitted with a conductor to each, might possibly be exposed at a moment when protection was most needed.

Professors Faraday and Wheatstone were of opinion, "that extreme cases should be provided for," and we would therefore recommend that the three masts as well as the bowsprit should be always fitted with the conductors.

While concluding our remarks on the mode of applying Mr. Harris's conductors, we beg to state to their Lordships, that though it is our decided conviction that no danger is to be feared from the assumed lateral explosion, yet if it be deemed advisable, for the sake of obviating any doubts which may still exist in the minds of some, we see no objection whatever to the copper plates on the fore-mast being placed on the fore

part of the mast, whereby the mast itself will intervene between the conductors and the magazine.

Having now completed our remarks on the several points to which their Lordships' instructions directed our attention, we trust we have shewn, from the evidence of facts derived from the experience of many years, as well as by the opinions not only of scientific, but professional, men, the efficacy of Mr. Harris's lightning conductors; and considering the number of lives which have been lost by lightning; the immense amount of property which has been destroyed, as shewn by Mr. Harris, and is still exposed without adequate protection; the inconvenience which has arisen, and is still liable to arise, from the loss of the services of ships at moments of great critical importance; the difficulty of procuring new spars in times of war on foreign stations, (not to mention the great expense of wages and victuals for the crews of ships while rendered useless till repaired; we again beg to state our unanimous opinion of the great advantages possessed by Mr. Harris's conductors above every other plan, affording permanent security at all times, and under all circumstances, against the injurious effects of lightning, effecting this protection without any nautical inconvenience or scientific objection whatever, and we therefore most earnestly recommend their general adoption in the Royal Navy,

We have &c. (signed)

A. M. GRIFFITHS, Rear-Admiral.

JAS. A. GORDON, Rear-Admiral.

JAS. CLARKE ROSS, Captain.

J. F. DANIELL, Professor of Chemistry.

JNO. FINCHAM, Master Shipwright.

II. *A Letter to Professor Faraday, on certain Theoretical Opinions.* By R. HARE, M. D., Professor of Chemistry in the University of Pennsylvania.*

Dear Sir,

I have been indebted to your kindness for several pamphlets comprising your researches in electricity, which I have perused with the greatest degree of interest.

You must be too well aware of the height at which you stand, in the estimation of men of science, to doubt that I

* Communicated by the Author.

entertain with diffidence, any opinion in opposition to yours. I may say of you as in a former instance of Berzelius, that you occupy an elevation inaccessible to unjustifiable criticism. Under these circumstances, I hope that I may, from you, experience the candor and kindness which were displayed by the great Swedish chemist in his reply to my strictures on his nomenclature.

I am unable to reconcile the language which you hold in paragraph 1615, with the fundamental position taken in 1155. Agreeably to the latter, you believe ordinary induction to be the action of *contiguous* particles, consisting of a species of polarity, instead of being an action of either particles or masses at "*sensible distances*." Agreeably to the former, you conceive that "assuming a perfect vacuum was to intervene in the course of the line of inductive action, it does not follow from this theory that the line of particles on opposite sides of such a vacuum would not act upon each other." Again, supposing "it possible for a positively electrified particle to be in the centre of a vacuum an inch in diameter, nothing in my present view forbids that the particle should act at a distance of half an inch on all the particles forming the disk of the inner superficies of the bounding sphere."

Laying these quotations before you for reconsideration, I beg leave to inquire how a positively excited particle, situated as above described, can react "inductrically" with any particles in the superficies of the surrounding sphere, if this species of reaction require that the particles between which it takes place be contiguous. Moreover if induction be not "an action either of particles or masses at *sensible distances*," how can a particle situated as above described, "*act at the distance of half an inch on all the particles forming the disk of the inner superficies of the bounding sphere?*" What is a sensible distance, if half an inch is not?

How can the force thus exercised obey the "well known law of the squares of the distances," if as you state (1375) the rarefaction of the air does not alter the intensity of the inductive action? In proportion as the air is rarefied, do not its particles become more remote?

Can the ponderable particles of a gas be deemed contiguous in the true sense of this word, under any circumstances? And it may be well here to observe, that admitting induction to arise from an affection of intervening ponderable atoms, it is difficult to conceive that the intensity of this affection will be inversely as their number as alleged by you. No such law holds good in the communication of heat. The air in contact with

a surface at a constant elevation of temperature, such for instance as might be supported by boiling water, would not become hotter by being rarefied, and consequently could not become more efficacious in the conduction of heat from the heated surface to a colder one in its vicinity.

As soon as I commenced the perusal of your researches on this subject, it occurred to me that the passage of electricity through a vacuum, or a highly rarefied medium, as demonstrated by various experiments, and especially those of Davy, was inconsistent with the idea that ponderable matter could be a necessary agent in the process of electrical induction. I therefore inferred that your efforts would be primarily directed to a re-examination of that question.

If induction, in acting through a vacuum, be propagated in right lines, may not the curvilinear direction which it pursues, when passing through "dielectrics," be ascribed to the modifying influence which they exert?

If, as you concede, electrified particles on opposite sides of a vacuum can act upon each other, wherefore is the received theory of the mode in which the excited surface of a Leyden jar induces in the opposite surface, a contrary state, objectionable?

As the theory which you have proposed, gives great importance to the idea of polarity, I regret that you have not defined the meaning which you attach to this word. As you designate that to which you refer, as a "species of polarity," it is presumable that you have conceived of several kinds with which ponderable atoms may be endowed. I find it difficult to conceive of any kind which may be capable of as many degrees of intensity as the known phenomena of electricity require; especially according to your opinion that the only difference between the fluid evolved by galvanic apparatus and that evolved by friction, is due to opposite extremes in quantity and intensity; the intensity of electrical excitement producible by the one, being almost infinitely greater than that which can be produced by the other. What state of the poles can constitute quantity—what other state of intensity, the same matter being capable of either electricity, as is well known to be the fact? Would it not be well to consider how, consistently with any conceivable polarization, and without the assistance of some imponderable matter, any great difference of intensity in inductive power, can be created?

When by friction the surface is polarized so that particles are brought into a state of constraint from which they endeavor to return to their natural state, if nothing be superadded to them, it must be supposed that they have poles capable of

existing in two different positions. In one of these positions, dissimilar poles coinciding, are neutralized ; while in the other position, they are more remote, and consequently capable of acting upon other matter.

But I am unable to imagine any change which can admit of gradations of intensity, *increasing* with remoteness. I cannot figure to myself any reaction which increase of distance would not lessen. Much less can I conceive that such extremes of intensity can be thus created, as those of which you consider the existence as demonstrated. It may be suggested that the change of polarity produced in particles by electrical inductions, may arise from the forced approximation of reciprocally repellent poles, so that the intensity of the inductive force, and of their effort to return to their previous situation, may be susceptible of the gradation which your electrical doctrines require. But could the existence of such a repellent force be consistent with the mutual cohesion which appears almost universally to be a property of ponderable particles ? I am aware that, agreeably to the ingenious hypothesis of Mossotti, repulsion is an inherent property of the particles which we call ponderable ; but then he assumes the existence of an imponderable fluid to account for cohesion ; and for the necessity of such a fluid to account for induction it is my ultimate object to contend. I would suggest that it can hardly be expedient to ascribe the phenomena of electricity to the polarization of ponderable particles, unless it can be shewn that if admitted, it would be competent to produce all the known varieties of electric excitement, whether as to its nature or energy.

If I comprehend your theory, the opposite electrical state induced on one side of a coated pane, when the other is directly electrified, arises from an affection of the intervening vitreous particles, by which a certain polar state caused on one side of the pane, induces an opposite state on the other side. Each vitreous particle having its poles severally in opposite states, they are arranged as magnetized iron filings in lines ; so that alternately opposite poles are presented in such a manner that all of one kind are exposed at one surface, and all of the other kind at the other surface. Agreeably to this or any other imaginable view of the subject, I cannot avoid considering it inevitable that each particle must have at least two poles. It seems to me that the idea of polarity requires that there shall be in any body possessing it, two opposite poles. Hence you correctly allege that agreeably to your views it is impossible to charge a portion of matter with

one electric force without the other. (*See par. 1177.*) But if all this be true, how can there be a "positively excited particle?" (*See par. 1616.*) Must not every particle be excited negatively, if it be excited positively? Must it not have a negative, as well as a positive pole?

I cannot agree with you in the idea that consistently with the theory which ascribes the phenomena of electricity to one fluid, there can ever be an isolated existence either of the positive or negative state. Agreeably to this theory, any excited space, whether minus or plus, must have an adjoining space relatively in a different state. Between the phenomena of positive and negative excitement there will be no other distinction than that arising from the direction in which the fluid will endeavor to move. If the excited space be positive, it must strive to flow outward; if negative, it will strive to flow inward. When sufficiently intense, the direction will be shewn by the greater length of the spark, when passing from a small ball to a large one. It is always longer when the small ball is positive, and the large one negative, than when their positions are reversed.*

But for any current it is no less necessary that the pressure should be on one side comparatively minus, than that on the other side, it should be comparatively plus; and this state of the forces must exist whether the current originates from a hiatus before, or from pressure behind. One current cannot differ essentially from another, however they may be produced.

In paragraph 1330, I have been struck with the following query, "What then is to separate the principle of these extremes, perfect conduction and perfect insulation, from each other; since the moment we leave the smallest degree of perfection at either extremity, we involve the element of perfection at the opposite ends?" Might not this query be made with as much reason in the case of motion and rest, between the extremes of which there is an infinity of gradations? If we are not to confound motion with rest, because in proportion as the former is retarded, it differs less from the latter; wherefore should we confound insulation with conduction, because in proportion as the one is less efficient, it becomes less remote from the other?

In any case of the intermixture of opposite qualities, may it not be said in the language which you employ "the moment we leave the element of perfection at one extremity, we in-

* See vol. 1. p. 489, of these Annals.

volve the element of perfection at the opposite." Might it not be said of light and darkness, or of opaqueness and translucency; in which case to resort to your language again, it might be added "especially as we have not in nature, a case of perfection at one extremity or the other." But if there be not in nature, any two bodies of which one possesses the property of perfectly resisting the passage of electricity, while the other is endowed with the faculty of permitting its passage without any resistance; does this affect the propriety of considering the qualities of *insulation* and conduction in the abstract, as perfectly distinct, and inferring that so far as matter may be endowed with the one property, it must be wanting in the other?

Have you ever known electricity to pass through a pane of sound glass? My knowledge and experience create an impression that a coated pane is never discharged through the glass unless it be cracked or perforated. That the property by which glass resists the passage of electricity, can be confounded with that which enables a metallic wire to permit of its transfer, agreeably to Wheatstone's experiments, with a velocity greater than that of the solar rays, is to my mind inconceivable.

You infer that the residual charge of a battery arises from the partial penetration of the glass by the opposite excitements. But if glass be penetrable by electricity why does it not pass through it without a fracture or perforation?

According to your doctrine, induction consists "in a forced state of polarization in contiguous rows of the particles of the glass" (1300); and since this is propagated from one side to the other, it must of course exist equally at all depths. Yet the partial penetration suggested by you, supposes a collateral affection of the same kind, extending only to a limited depth. Is this consistent? Is it not more reasonable to suppose that the air in the vicinity of the coating gradually relinquishes to it a portion of free electricity, conveyed into it by what you call "*convection*." The coating being equally in contact with the air and glass, it appears to me more easy to conceive that the air might be penetrated by the excitement, than the glass.

In paragraph 1300, I observe the following statement: "*When a Leyden Jar is charged, the particles of the glass are forced into this polarized and constrained condition by the electricity of the charging apparatus. Discharge is the return of the particles to their natural state, from their state of tension, whenever the two electric forces are allowed to be disposed of in some other direction.*" As you have not previously mentioned any particular direction in which the forces

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are exercised during the prevalence of this constrained condition, I am at a loss as to what meaning I am to attach to the words "some other direction." The word *some*, would lead to the idea that there was an uncertainty respecting the direction in which the forces might be disposed of; whereas it appears to me that the only direction in which they can operate, must be the opposite of that by which they have been induced.

The electrified particles can only "return to their natural state" by retracing the path by which they departed from it. I would suggest that for the words "*to be disposed of in some other direction,*" it would be better to substitute the following, "*to compensate each other by an adequate communication.*"

Agreeably to the explanation of the phenomenon of coated electrics afforded in the paragraph above quoted (1300), by what process can it be conceived that the opposite polarization of the surfaces can be neutralized by conduction through a metallic wire? If I understand your hypothesis correctly, the process by which the polarization of one of the vitreous surfaces in a pane produces an opposite polarization in the other, is precisely the same as that by which the electricity applied to one end of the wire extends itself to the other end.

I cannot conceive how two processes severally producing results so diametrically opposite as insulation and conduction, can be the same. By the former, a derangement of the electric equilibrium may be permanently sustained, while by the other, all derangement is counteracted with a rapidity almost infinite. But if the opposite charges are dependent upon a polarity induced in contiguous atoms of the glass, which endures so long as no communication ensues between the surfaces; by what conceivable process can a perfect conductor cause a discharge to take place, with a velocity at least as great as that of the solar light? Is it conceivable that all the lines of "contra-induction" or depolarization can concentrate themselves upon the wire from each surface so as to produce therein an intensity of polarization proportioned to the concentration; and that the opposite forces resulting from the polarization are thus reciprocally compensated? I must confess, such a concentration of such forces or states, is to me difficult to reconcile with the conception that it is at all to be ascribed to the action of the rows of *contiguous ponderable particles*.

Does not your hypothesis require that the metallic particles, at opposite ends of the wire, shall in the first instance be subjected to the same polarization as the excited particles of the glass; and that the opposite polarizations, transmitted to

some intervening point, should thus be mutually destroyed, the one by the other? But if discharge involves a return to the same state in vitreous particles, the same must be true in those of the metallic wire. Wherefore then are these dissipated, when the discharge is sufficiently powerful? Their dissipation must take place either while they are in the state of being polarized, or in that of returning to their natural state. But if it happen when in the first mentioned state, the conductor must be destroyed before the opposite polarization upon the surfaces can be neutralized by its intervention. But if not dissipated in the act of being polarized, is it reasonable to suppose that the metallic particles can be sundered by returning to their *natural state* of depolarization?

Supposing that ordinary electrical induction could be satisfactorily ascribed to the reaction of ponderable particles, it cannot, it seems to me, be pretended that magnetic and electro-magnetic induction is referable to this species of reaction. It will be admitted that the Faradian currents do not for their production require intervening ponderable atoms.

From a note subjoined to page 37 of your pamphlet, it appears that "on the question of the existence of one or more imponderable fluids as the cause of electrical phenomena, it has not been your intention to decide." I should be much gratified if any of the strictures in which I have been so bold as to indulge, should contribute to influence your ultimate decision.

It appears to me that there has been an undue disposition to burden the matter, usually regarded as such, with more duties than it can perform. Although it is only with the properties of matter that we have a direct acquaintance, and the existence of matter rests upon a theoretic inference that since we perceive properties, there must be material particles to which those properties belong; yet there is no conviction which the mass of mankind entertain with more firmness than that of the existence of matter in that ponderable form, in which it is instinctively recognized by people of common sense. Not perceiving that this conviction can only be supported as a theoretic deduction from our perception of the properties; there is a reluctance to admit the existence of other matter, which has not in its favor the same instinctive conception, although theoretically similar reasoning would apply. But if one kind of matter be admitted to exist because we perceive properties, the existence of which cannot be otherwise explained, are we not warranted, if we notice more properties than can reasonably be assigned to one kind of matter, to assume the existence of another kind of matter?

Independently of the considerations which have heretofore led some philosophers to suppose that we are surrounded by an ocean of electric matter, which by its redundancy or deficiency, is capable of producing the phenomena of mechanical electricity, it has appeared to me inconceivable that the phenomena of galvanism and electro-magnetism, latterly brought into view, can be satisfactorily explained without supposing the agency of an intervening imponderable medium by whose subserviency the inductive influence of currents or magnets is propagated.* If in that wonderful reciprocal reaction between masses and particles, to which I have alluded, the polarization of condensed or accumulated portions of intervening imponderable matter, can be brought in as a link to connect the otherwise imperfect chain of causes; it would appear to me a most important instrument in lifting the curtain which at present hides from our intellectual vision, this highly important mechanism of nature.

Having devised so many ingenious experiments tending to show that the received ideas of electrical induction are inadequate to explain the phenomena without supposing a modifying influence in intervening ponderable matter, should there prove to be cases in which the results cannot be satisfactorily explained by ascribing to them ponderable particles, I hope that you may be induced to review the whole ground, in order to determine whether the part to be assigned to contiguous ponderable particles, be not secondary to that performed by the imponderable principles by which they are surrounded.

But if galvanic phenomena be due to ponderable matter, evidently that matter must be in a state of combination. To what other cause than an intense affinity between it and the metallic particles with which it is associated, can its confinement be ascribed consistently with your estimate of the enormous quantity which exists in metals? If "a grain of water or a grain of zinc, contain as much of the electric fluid as would supply eight hundred thousand charges of a battery containing a coated surface of fifteen hundred square inches," how intense must be the attraction by which this matter is confined? In such cases, may not the material cause of elec-

* This view is precisely that which we have given at page 270, vol. 1, of these Annals; and, consequently, in strict conformity with the theory of electro-magnetism and magnetic-electricity, which we have there explained. We are glad to find an acknowledgement of the principles of our theory from so eminent an experimental philosopher as Dr. Hare; it gives us some hopes that it will soon be more generally acknowledged as affording the simplest and most natural mode of explaining electro-magnetic and magnetic-electrical phenomena, that has hitherto been offered to the notice of philosophers.—Edit.

tricity be considered as latent, agreeably to the suggestion of *Ørsted*, the founder of electro-magnetism? It is in combination with matter, and only capable of producing the appropriate effects of voltaic currents when in act of transfer from combination with one atom to another; this transfer being at once an effect and a cause of chemical decomposition, as you have demonstrated.

If polarization, in any form, can be conceived to admit of the requisite gradations of intensity, which the phenomena seem to demand; would it not be more reasonable to suppose that it operates by means of an imponderable fluid existing throughout all space, however devoid of other matter? May not an electric current, so called, be a progressive polarization of rows of the electric particles, the polarity being produced at one end and destroyed at the other incessantly, as I understood you to suggest in the case of contiguous ponderable atoms.

When the electric particles within different wires are polarized in the same tangential direction, the opposite poles being in proximity, there will be attraction. When the currents of polarization move oppositely, similar poles coinciding, there will be repulsion. The phenomena require that the magnetized or polarized particles should be arranged as tangents to the circumference, not as radii to the axis. Moreover, the progressive movement must be propagated in spiral lines in order to account for rotatory influence.

Between a wire which is the mean of a galvanic discharge, and another not making a part of a circuit, the electric matter which intervenes may, by undergoing a polarization, become the medium of producing a progressive polarization in the second wire moving in a direction opposite to that in the inducing wire; or in other words an electrical current of the species called Faradian may be generated.

By progressive polarization in a wire, may not stationary polarization, or magnetism be created; and reciprocally by magnetic polarity, may not progressive polarization be excited?

Might not the difficulty, above suggested, of the incompetency of any imaginable polarization to produce all the varieties of electrical excitement which facts require for explanation, be surmounted by supposing intensity to result from an accumulation of free electric polarized particles, and quantity from a still greater accumulation of such particles, polarized in a latent state or in chemical combination?

There are, it would seem, many indications in favour of the idea that electric excitement may be due to a forced polarity, but in endeavouring to define the state thus designated,

or to explain by means of it the diversities of electrical charges, currents and effects, I have always felt the incompetency of any hypothesis which I could imagine. How are we to explain the insensibility of a gold leaf electroscope, to a galvanized wire, or the indifference of a magnetic needle to the most intensely electrified surfaces?

Possibly the Franklinian hypothesis may be combined with that above suggested, so that an electrical current may be constituted of an imponderable fluid in a state of polarization, the two electricities being the consequence of the position of the poles or their presentation. Positive electricity may be the result of an accumulation of electric particles, presenting poles of one kind; negative, from a like accumulation of the same matter with a presentation of the opposite poles, inducing of course an opposite polarity. The condensation of the electric matter, within ponderable matter, may vary in obedience to a property analagous to that which determines the capacity for heat, and the different influence of dialectrics upon the process of electrical induction may arise from this source of variation.

With the highest esteem, I am yours truly,

ROBERT HARE.

III.—*Description of a New Electro-tome.* By
THOMAS WRIGHT, Esq. In a Letter to the Editor.

Sir,

I have lately contrived a self-acting Electro-tome, an account of which I send you herewith, in hope that you may think it worthy of insertion in your valuable journal.

I am, Sir,

Yours respectfully,

THOMAS WRIGHT.

Knutsford, 24th March, 1840.

In fig. I. pl. 1, A is the end of an electro-magnetic coil, to which it is desired to apply the Electro-tome, B is a block of wood morticed into the foot-board of the machine, through which pass the small brass bars C D and E F; at D is soldered a copper wire, which is bent round the end of the bar E F, so as to form a spring pressing on the same at G; to the end of the spring at G is soldered a piece of tinned iron H, one inch broad and three inches long, bent in a U form as shewn at I. On the bar C D is a binding screw at D, and one end of the primal wire of the coil is soldered to E.

To put the machine in action, one of the poles of a battery is screwed into D, and the other to the unattached end of the

primal wire; by this means the current passes along D G E, through the coil, and out at L, magnetizing the core of iron wires in the coil, these immediately attract the piece of tinned iron H, and thus contact is broken at G with inconceivable rapidity: indeed, when the spring is bent close, and the magnet is approximated very nearly to the wires, the motion of the break produces a continued loud hum; the shocks however are not so strong as when the magnet is further from the break, and more time thus allowed for the developement of magnetism in it.

The electro-magnet which I employ is formed of a bundle of annealed iron wires, two feet long and about half an inch in diameter; over this is coiled forty yards of copper wire, 1-10th of an inch thick, (this is too thick when a small battery is intended to be used,) and over this, soldered to it, and forming one continuous coil, are wrapped 100 yards of copper wire, 1-15th of an inch thick; the battery connexions are made with the ends of the primal or thick wires; the shocks and decompositions are obtained from the whole length.

The primal current of this magnet gives a strong shock; (even with three square inches of zinc in the battery described hereafter;) the shock from the whole length is however intolerable, when the ends of the wires are touched with small pieces of tinned iron held between the dry tips of the finger and thumb of each hand; and when the cylindrical conductors attached to the machine are held loosely with dry hands, a continued crackling is heard, as if the electric fluid was passing through the thin space of air between the conductors and the hands.*

Mercury must not on any account be used as part of the electro-tome to the above coil, as the greater part of the secondary current passes in the dense and *vaporous* spark, occasioned by the combustion of that metal.

Instead of copper in the battery, I use common tea-chest lead, which I find to answer better. It is prepared by cutting strips of it in the form of a feather, so as to offer a great number of points for the conduction of the electric fluid, these are then placed round the inside of a jar, and the ends turned over the rim, and bound in their places with a bright copper wire firmly twisted round them, to serve as negative conductor. The zinc and diaphragms as in Daniels, the solutions as in Mullin's batteries.

Since the receipt of this letter, we have been favored with a sight of Mr. Wright's very neat instrument, at the Royal Victoria Gallery of Practical Science, Manchester; and can testify as to its powers fully answering to the description.—*EDIT.*

IV.—*Description of a New Electro-magnetic Machine*
By THOMAS WRIGHT, Esq. In a Letter to the Editor.

Dear Sir,

The following description of an Electro-magnetic Machine will, I hope, prove interesting to the readers of your "Annals."

I am

Yours respectfully,

THOMAS WRIGHT.

Knutsford, March 28, 1840.

Fig. 2, gives a sectional view of the apparatus. It consists of two concentric cylinders of copper joined together at the bottom; the inner one A encloses an electro-magnetic coil, the end of the bobbin appearing at the top. The dotted lines represent concentric cylinders of porous earthenware, with a cylinder of zinc between them. To the cylinder A is soldered a wire at B, bearing a U shaped piece of tinned iron at its extremity, and pressing with a slight spring against C, which is a bent brass bar in conjunction with one end of the primal coil. D is the other end of the primal coil attached by a binding screw to the zinc; this is a modification of my self-acting electro-tome. The primal and thin coils are joined to form one coil: the end of the thin coil is attached to the binding screw on the bobbin at E. The shock is taken from thin spirals attached to C and E; sparks, &c, from wires attached to C and D.

Professor Henry has found that electric induction does not take place (or very imperfectly) when a closed circuit is interposed between the primal and superimposed coils; but I have not seen it remarked that magnetic induction follows the same law. The following experiment however seems to shew that it does:—

Being desirous of getting rid of the oxidizing spark which takes place on the breaking of the battery current, I joined the two brass bars of the electro-tome, described in my last letter with a fine iron wire two yards long,* in hopes that the secondary current, on account of its superior intensity, would pass through it, though the battery current could not. This answered my expectation fully; the break continued to work as usual, and the spark disappeared entirely. [This experiment was performed in the dark.]

* I am afraid I shall be thought pedantic if I call this wire the "deuterode," or "path of the secondary" current, but as I hope to make it a source of utility in another course of experiments, I have thought it advisable to give it a specific name.

I then took hold of the ends of the super-imposed coil, and was much surprised to find that it did not give the slightest shock, even when its extremities were placed in the mouth: thinking that the insulation must be imperfect, I disjoined the "denterode," the shocks were then as powerful as usual, but on joining it again they instantly ceased.

V.—*Description of an Electro-magnetic Engine.*—
By MR. URIAH CLARKE. In a Letter to the Editor.

Sir,

Enclosed I send you (for publication in the *Annals*, if you please,) a drawing and description of, as I think, an original mode of applying voltaic agency for the purpose of acquiring motive power. Among the varied forms of rotatory movements, hitherto made public, I believe the general principle has been to apply the magnetic action in a *lateral* manner. This circumstance, together with the very limited sphere of action of even the most powerful magnets, has, perhaps, been the cause why large machines and small ones have been so disproportioned in their effects, that the latter seem to furnish no data for calculating the power of the former.

In the machine which I am about to describe to you, the magnet is made to act in a *direct* manner upon a reciprocating bar, which bar communicates its motion to a crank, just at the most favorable point, viz., when it (the crank) is nearly in a horizontal position, and when the bar is making a close approach to the magnet; consequently, the most intense action of the magnet is applied to the crank in that part of its revolution where it is most effective. Now, a slight consideration of the principle of the crank will shew the importance of this object, which is obtained by joining the reciprocating bar to the crank, by means of a chain, or other flexile communication. (How far this principle may be applicable for maximizing the effect of a given power upon cranks generally, I leave with mechanics to determine—it is indispensable here.)—When the crank has moved below the horizontal position, the reciprocating bar falls upon a rest, which prevents any percussion taking place on the ends of the magnet. The bar remains upon the rest until the crank has made the lower part of its revolution, during which time contact with the battery is broken, and the bar, of course, is disengaged from the magnet; it (the bar) is then lifted by the crank, as it passes over the centre, when battery communication being again made, another impulse is given. It will, no doubt, occur to

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you, that a number of magnets may be used with a succession of cranks. I trust that figure 3, plate 1, will be sufficiently intelligible. A is the chain communicating between the bar and crank. B is the reciprocating bar, and C is the electro-magnet. The break is here omitted to avoid confusion. I effect it by means of a small eccentric upon the shaft of the fly-wheel. I would observe, in conclusion, that the above contrivance is not a mere embryo of the mind; for I have now a working model in actual operation, which I invented nearly two years ago. It has been at work occasionally for the last eighteen months, and, as I have made no secret of it, it has been seen by a great number of professional gentlemen and scientific friends, amongst whom I take the liberty of naming a friend and correspondent of yours, Mr. James Mitchell, of this place.

I am, Sir,
Very respectfully yours,
URIAH CLARKE.

Leicester, March 16th, 1840.

VI.—On the new Metal Lantanum. By JAMES C. BOOTH
AND CAMPBELL MORFIT.

[From the Journal of the Franklin Institute.]

A notice of the discovery of this element having already appeared in one of our Scientific Journals, it occurred to us that an account of some of our experiments with it might present a subject of sufficient interest to the readers of the Institute Journal.

The name is derived from the Greek *λαντανειν*, to lie hid;* it is called in Swedish and German, Lantan, but in English Lantanum, for the sake of euphony and in accordance with the generally received termination of the names of the elements. The ordinary method of obtaining cerium by precipitation with the bisulphate of potassa, threw down a bi-salt of Lantanum at the same time, the latter constituting two-fifths of the whole saline mass. The method of separating the two depends on the ready solubility of oxide of lantanum in dilute acid after ignition, a property lost by cerium under the same circumstances. From its nitric solution, it may be best thrown down as a white, crystalline carbonate, by carbonate of ammonia, and from this its other compounds may be formed. The dry chloride heated with potassium was reduced to a grey metallic powder possessing a dark lead-color, and capa-

* From its concealment hitherto in the compounds of cerium.

ble of being flattened together by pressing. It is slowly converted into oxide in the air, and in cold water into a hydrated oxide with the evolution of hydrogen. An effervescence takes place in hot water.

It has two isomeric states. The ordinary salts possess a faint reddish tinge, but when the yellowish red oxide is heated in hydrogen gas, it becomes white with a faint shade of green, and dissolves with more difficulty in acids, forming salts which possess a greenish hue.

With bisulphate of potassa it forms a slowly soluble salt, which, however, does not precipitate like the corresponding salt of cerium, unless the latter be also present in the solution. Its atomic weight is lower than that adopted for the oxide of cerium.

The above notice is mainly extracted from Berzelius' letter to Poggendorff, published in Nos. 4 and 5 of Poggend. Annals for the present year. Our experiments were as follows:

Having prepared the sulphate of cerium and potassa by the ordinary methods from the mineral cerite, it was dissolved in a large quantity of boiling water, and the hydrated oxides of cerium and lanatium precipitated by caustic potassa. These were dissolved in nitric acid after being thoroughly washed, evaporated to dryness, and heated in a platinum crucible until all the nitric acid was expelled. The oxides remained of a light reddish brown color, and were transferred to a glass containing nitric acid diluted with 60 to 80 times as much water. After digesting about two hours in a gentle warmth, the lanatium was dissolved and oxide of cerium remained of a reddish-brown color. The solution treated with caustic potassa threw down the white hydrated oxide of lanatium, much more bulky and gelatinous in appearance than alumina. It is exceedingly difficult, if not impossible, to wash it out thoroughly, for after edulcoration for several days, the liquid passing through the filter still gave indications of a solid matter, and almost led to the belief that the oxide was slightly soluble in water.

On re-dissolving hydrated oxide in nitric acid, evaporating to dryness, and heating to redness, the dry oxide remained of a brick-red color, differing therefore from the oxide of cerium by a lighter hue, and by containing less of a brownish shade. On treating this oxide as before with very dilute nitric acid, a small portion of oxide of cerium remained, proving that this mode of separating the two metals is not accurate, and that we must await further experiments for the discovery of a more perfect method.

Carbonate of lanatium, as thrown down by carbonate

of soda, is a voluminous white precipitate, and, like the hydrated oxide, very difficult of edulcoration, for after obtaining the chloride from it, crystals of common salt were also visible. Agreeably to the observations of Mosander, therefore, the carbonate of ammonia is the best precipitant.

Sulphate of lanatium is readily formed by the solution of the oxide, or carbonate, in dilute sulphuric acid, evaporation to a small bulk by heat and exposure to self-evaporation, while delicate needles of a flesh-red color collect in little groups on the bottom of the capsule.

The chloride is similarly formed by means of chloro-hydric acid and evaporation. It forms a light yellowish green crystalline mass, in which no determinate form was observed.

The quantity of lanatium in our possession was so small, amounting only to a few grains, that the operations were necessarily conducted slowly, and prevented our pursuing them quantitatively. Should we be enabled to obtain a larger amount, we may give more interesting results, without, however, trespassing on the field legitimately belonging to the discoverer.

VII.—Remarks on the Imperfect Elasticity of Glass Threads used in Torsion Instruments. By W. H. GOODE, Chemical Assistant in the Laboratory of the University of the City of New York.

[From the Journal of the Franklin Institute.]

From the superior elasticity which glass enjoys over most substances, particularly the metals, threads of this material have replaced the metallic wires with which the needles of torsion instruments were at first suspended. The late Dr. Ritchie employed them in his improved torsion galvanometer, which emulates the torsion balance of Coulomb, for accuracy in measuring small forces. It is necessary, however, to the perfection of torsion instruments, that the elasticity of the thread of suspension, though a feeble, should be a constant force; and also that the thread itself should suffer no alteration of its conditions by the amount of force exerted upon it. Perfectly elastic substances only, fulfil these requirements; on all imperfectly elastic bodies torsion acts irregularly, and impresses a change upon them which prevents their return to their normal position, after it has been removed; such, for example, is the change effected in the conditions of metallic wires, filaments of silk, hair, &c., which, being imperfectly elastic, fail to return to zero when released from torsion.

The impression has become general that threads of glass, of a certain degree of tenuity, unlike the substances already mentioned, are not permanently affected by force exerted on them; but are capable always of regaining their original position; their elasticity is therefore considered to be perfect,—and measures effected with instruments to which they are adjusted, rigidly accurate. Having observed that the needle of a galvanometer suspended by a glass thread did not return to zero, after the instrument had been employed in a series of observations, it became a subject of enquiry to ascertain the source of error. For this purpose, threads of different diameters were suspended, in a manner similar in all respects, to that of a torsion balance. Two small uprights were placed on opposite sides of the little glass needle, near its opposite ends, which served as obstacles to it when torsion was made, and prevented it from rotating along with the micrometer. On some point of the thread, another little glass needle, carrying an upright arm, was cemented. This arm served as an index for the thread, and marked its position on a scale pasted on the opposite side of the jar; it was observed through a small hole in a plate of metal, placed ten or fifteen inches distant;—an improved method of observing a vertical index introduced by Professor Draper.

The thread being freely suspended, its index and that of the micrometer at their respective zeros, a torsion of three revolutions of the micrometer was exerted on the thread for five minutes; when released from this force it did not resume its position at zero, but varied from it in the direction opposite to that in which the torsion had been made. If the micrometer be now kept at zero, it will be found that the thread partially recovers itself, after the lapse of several hours; the amount of error is consequently diminished, but it never returns accurately to its original position. If instead of releasing the thread by returning the micrometer to zero, the thread be released from the obstacles, the same result will be obtained; in consequence of the impetus it requires in spinning round, this latter is probably the less accurate method of observation.

A variety of experiments were performed with different threads, to ascertain if the alteration of the zero bore any constant relation to the amount of torsion employed. If such were the case, a system of compensation could be adopted which would free instruments fitted with glass threads from error.

The result of the observations made with one thread are given, for convenience, in a tabular form; they are analogous

38 *On the Imperfect Elasticity of Glass Threads.*

to those obtained with others; and indicate that the amount of the alteration of the zero, for the same threads, for different degrees of torsion, bears no constant proportion to the amount of force employed.

TABLE I.

	No. of degrees of Torsion.	Error of Zero.	Duration of Torsion.	Thermometer
1	360°	2°	5'	70°
2	720°	10°	5'	70°
3	1080°	13°	5'	70°
4	1440°	20°	5'	70°

The thermometer was carefully noted in these experiments, lest the temperature of the room should vary during the prosecution of them; for it is not known what influence changes of temperature may exert on the elasticity of glass. After a lapse of twelve hours the thread had not returned to zero: another zero being assumed the following table of errors was obtained;

TABLE II.

	No. of degrees of Torsion.	Error.	Duration of Torsion.	Thermometer
1	360°	2°	5'	70°
2	360°	3°	5'	70°
3	360°	3°	5'	70°
4	720°	6°	5'	70°
5	1080°	9°	5'	70°
6	1440°	12°	5'	70°

The thread was now left freely suspended for two hours; another needle and index were then adjusted to it, parallel to the first, but at twice the distance of the first from the micrometer: two points of the thread could now be observed and the effect of torsion on a double length noted. Up to 1080 the error of its two indexes was the same in amount; for that degree of torsion, the farther index varied more from its zero than the nearer, and consequently required the micrometer to pass through a greater space to restore it to that point. For a torsion of 1440° the error of the more distant index from the micrometer exceeded that of the nearer, three degrees. The tabular results are as follows;

TABLE III.

	No. of degrees of Torsion.	Error.	Duration of Torsion.	Thermometer
1	360°	3°	5'	70°
2	720°	6°	5'	70°
3	1080°	6°	5'	70°
4	1440°	*	5'	70°

* Upper Index 5°. Lower one 8°.

The result indicated in the last experiment of the preceding table, has been observed to take place repeatedly when high degrees of torsion have been employed and continued for a long period. It is probably due to slight inequalities in the diameter of the threads and to their being differently annealed in different portions of their length; force exerted on them would, under such circumstances, produce more decided effects on some portions than on others, and the effort made by the thread to recover itself would also occasion in different parts of it, different degrees of error.

An inspection of these tables will shew that the whole amount of error, for each series of observations, is decreasing; in the first it amounted 20°, in the second to 12°, in the third to 6°. It would therefore appear that a certain amount of torsion could be exerted on the thread for a certain period, for which it would afford no error of the zero. One thread which has been operated with, exhibited this effect of force exerted on it, in a very marked manner; for a torsion of two revolutions of the micrometer, its zero altered 10°, but for four revolutions there was no alteration. It was broken in making another observation.

For degrees of torsion less than that at which this effect takes place, the error of the zero will continue to exist; as it is observed, in the two last tables that for one and two revolutions of the micrometer, the error is nearly constant.

It has been shewn that a force amounting to one revolution of the micrometer exerted for five minutes, produced a permanent deflection of the zero of the thread, two degrees. An attempt was made to ascertain the influence of smaller degrees of force on the thread, exerted for a longer period. A torsion of 270° produced, in five minutes, a permanent deviation of the zero 1½°; 180° of torsion in the same period occasioned no error. A torsion of 90° continued for half an hour, produced an alteration of 2°; for 45° of torsion continued one and a half hours, it altered 3°. The permanent alteration of the zero of a glass

thread appears to be occasioned, either by a large amount of force exerted upon it, or by smaller forces acting for a comparatively long period. The amount of this alteration is modified by a variety of circumstances; the diameter of the thread—the uniformity with which it cooled in being drawn—the amount of force to which it has been subjected, and its duration—combine in producing this effect; which, influenced by so many causes, must necessarily be variable in amount.

It is not probable that any two threads will afford the same numerical value of their respective errors of zero, for the same force exerted on them. We cannot be certain that they are in precisely the same condition—that they are identical in composition, or that they are equally well annealed. Both, however, will fail to return to zero after a certain amount of torsion has been exerted on them, by a quantity which diminishes for every series of observations; and which for a certain amount of torsion, becomes nothing.

The effect of this error of the zero on instruments constructed for the purpose of measuring small forces by torsion, is to cause the deviations of the needle, when the thread is newly suspended to be less than they ought to be, and less than they ever are afterwards; in each succeeding observation the force acting on the needle has to overcome not only the forces which keep it in equilibrio, but that also which deflects the thread from its zero; as this latter increases with the increase of torsion and with the time it occupies, the last deflections of the needle are much less than the first of the same series.

This kind of error will be found in every series of experiments, though it will vary in amount; but it will still be sufficient if not corrected for, to vitiate results requiring to be obtained accurately. In using torsion instruments, after each observation, the error of the zero ought to be ascertained and the due correction applied. But where it can be done, it is advisable first to ascertain the amount of torsion necessary in five minutes to cause an alteration of the zero; and then to ascertain the period of time which that amount of torsion intended to be employed requires to produce the same effect, and not transcend these limits, by employing a greater amount of force on the thread, or using the instrument continuously for a longer time than it will afford correct measures.

VIII.—*On the Course of the Electrical Discharge, and on the Effects of Lightning on certain Ships of the British Navy, &c. &c.* By W. SNOW HARRIS, Esq. F.R.S.

In the instance I last quoted of damage to H.M.S. Rodney by lightning, it will be remembered that there was no regular metallic line through which the forces in action could become neutralized. The electrical agency had therefore to find for itself such a general course, as upon the whole opposed the least resistance to its progress; and it is evident that in this case its path was determined on the general principles before laid down in sec. 17.

25. I shall now proceed to state a few cases of damage to certain other ships of the navy, where metallic bodies happened to be so disposed about the rigging and hull, as to approximate in some measure to the conditions of experiment 2, sec. 18,* and consequently to that perfect state of defence against the expansive force of the electrical discharge in which a ship would become placed, by perfecting the conducting power of the masts, and uniting them into one general continuous system with the metallic masses in the hull, and with the sea.

These cases are particularly interesting, and conclusive of the general question of the protection to be afforded by such a system.

No. 1.—In September 1833, H.M. ship Hyacinth had both the fore and main-top masts and top-gallant masts destroyed by lightning in the Indian Ocean. The electric fluid shivered these masts from the truck to the heel of the topmast, as indicated by the waving black line *a b* in fig. 4. pl. I. which represents the effects on the main mast; at the point *b*, it became assisted by the chain topsail sheet leading to the deck at *c*, and so did no further damage to the mast; at *d* it received further assistance from the copper pipe of Hearle's patent pump, leading to a small well at *e*, and thence by a second pipe through the ship's side under water, and by this passed safely into the sea.†

26. Now it is evident here that a heavy discharge of lightning which shivered completely a sloop of war's main-top mast and top-gallant mast varying from 11 inches to a foot in dia-

* Annals of Electricity, &c. Vol. iv. p. 492.

† These circumstances are minutely detailed by Capt. Blackwood, who commanded the ship at the time, and may be seen in his interesting letter on the subject, in the Nautical Magazine, vol. viii., p. 116.

meter through a length of at least 80 feet, was conducted without damage or fusion by an iron chain and a short copper pipe. It is therefore important to state the dimensions of these metallic bodies. Now the iron chain consisted of links $2\frac{1}{4}$ inches long, made of iron rod $\frac{1}{2}$ inch in diameter. It reached from the lower yard to the deck, a distance of about 50 feet.

The pump consisted of copper pipe 4 pounds to the square foot; it was 3 inches in diameter, and about the $\frac{1}{16}$ th of an inch thick, extending through a distance of about 10 feet.

The effects on the foremast were very similar, they are omitted therefore for the sake of brevity.

27. It is not a little remarkable, that five years after this, in 1838, this same ship was again struck by lightning, whilst at anchor in Penang Bay, and again lost her main-top mast and top-gallant mast in a similar way, the lower mast being preserved by her chain topsail sheets.

28. No. 2.—In 1830, the Athol, of 28 guns, was struck by lightning on her foremast, in the Bight of Biafra: at this time the topsails were lowered on the caps and the other sails furled, as shewed in fig. 5. This ship had chains for hoisting the topsails which lay in the direction of the topmast as indicated by the dotted line *b c*. She had also a chain for topsail sheets, which led along the lower masts as indicated by the line *d e*. When the electrical explosion fell on the truck it shivered the top-gallant mast in pieces so far as the commencement of the chain at *b*; here being assisted by the chain, it passed on *without* any damage to the topmast, which is extremely worthy of remark, because in the former case, where there was no chain, the topmast was destroyed.

Having reached the point *c*, where the chain terminated, it passed *with* damage over the head of the mast, until again being assisted by the lower chain *d e*, it passed *without* damage to the deck; on reaching the deck at *e*, it passed by means of a bolt through a beam in the forecastle upon the chain cable, and thence into the sea.*

29. These effects are similar to the former, and shew the protection afforded by the chains, and their power of conducting heavy discharges of lightning without any of the ill consequences insisted on by Mr. Sturgeon; since in both cases the chains were in the vicinity of large metallic masses, viz. the iron hoops, iron-bound blocks, &c. about the masts, and

* An interesting and authentic account of this circumstance will be found in the Nautical Magazine, vol. viii., p. 114.

in both cases the lightning passed through the hull. Now as all the laws of nature are general, not partial, it is reasonable to infer, that if Mr. Sturgeon's view of a lateral explosion were true, it ought to apply in such papable cases as these, more especially when he says he can produce a lateral explosion at 50 feet distance with a jar of only "a quart of capacity."

30. No. 3.—The effects of lightning on H.M.S. Snake, is another striking instance of the general laws we have been contending for. The phenomena are detailed with peculiar clearness by Capt. Milne in the March number of the Nautical Magazine. The electric fluid entered main truck, shivered royal mast, splintered top-gallant mast; then over *chain* main topsail tye *without* damage to within 8 feet of the deck so far as the topsail halliards.

Finding, as observed by Capt. Milne, an obstruction here in the ropes, it again seized on the mast, and became divided at the saddle of main boom; one portion passed out of quarter-deck port to the sea, the other to lower deck and down the mast, and distributed itself over the hull, affecting persons below. The mast, on being examined at Halifax, was sprung about the partners 2 inches deep and 15 inches round, and was *perfectly burst asunder at the step*: hence the shock had extended to the heel, the electric matter, consequently, must have passed by the metallic bolts in the keelson to the sea.

It is further stated, and it is *a most important fact*, that a seaman *aloft on the cross trees*, at the time, did not experience any sensation whatever.

31. No 4.—The Buzzard brigantine was struck by lightning on the Coast of Africa, in February 1838, and lost her top-gallant and topmast, under precisely the same circumstances as those of the Hyacinth, the lower mast being preserved by the chain topsail sheet*.

32. No. 5.—The Fox revenue cutter was struck by lightning in March 1818. The mast was furrowed and otherwise damaged in every part *except where it was coppered*; as appears by a minute made at the time by the master mast-maker at the Plymouth dock-yard.

Now the copper usually placed about a cutter's mast is not the $\frac{1}{4}$ nd part of an inch in thickness. In this case it remained perfect.

33. No. 6.—The spire of a church at Kingsbridge in Devon-

* This case was given me by the commander Lieut. Fox. I was myself on board the vessel on her arrival. The particulars are noted in her log.

shire was struck by lightning in June 1828, and fearfully damaged. This case is particularly worthy of notice.

The lightning fell on an iron spill, *a*, *b*, fig 6. supporting the weather-cock, about 7 feet in length and 1 inch in diameter. On this it produced no visible effect, nor did *any damage arise to the stone-work about the rod*. It was not until the rod ceased at the point *b* that the masonry was rent.*

34. No. 7.—Extract from a letter from Lieut. Sullivan, of H.M.S. "*Beagle*," addressed to the Editor of the *Annals of Electricity, &c. &c.*, relative to the protection afforded by a continuous conductor attached to the mast of H.M.S. *Beagle*.

"Having considered your communication in the *Annals of Electricity* on marine lightning conductors, containing observations on the stroke of lightning which fell on the masts of H.M.S. *Beagle*, I think it fair, both to Mr. Harris and the naval service, to describe the phenomena I witnessed on that occasion; first stating, that at the time of my joining the *Beagle* in 1831, previously to her leaving England, I had no acquaintance with Mr. Harris. and certainly *no bias* in favour of the conductors with which the ship was fitted. I may therefore claim to be considered an impartial observer.

"At the time alluded to, I was first Lieutenant of the *Beagle*, and was attending to the duty on deck. She was at anchor off Monte Video, in the Rio de la Plata, a part of the world very often visited by severe lightning storms. Having been on board H.M.S. *Thetis* at Rio Janeiro a few years before, when her *foremast was entirely destroyed by lightning*, my attention was always particularly directed to approaching electrical storms, and especially on the occasion alluded to, as the storm was unusually severe. The flashes succeeded each other in rapid succession, and were gradually approaching; and I was watching aloft for them when the ship was apparently wrapt in a blaze of fire, accompanied by a *simultaneous* crash, which was equal if not superior to the shock I felt in the *Thetis*; one of the clouds by which we were enveloped had evidently burst upon the vessel, and as the mainmast appeared for the instant to be in a mass of fire, I felt certain that the lightning had passed down the conductor on that mast; the vessel was shaken by the shock, and an unusual tremulous motion could be distinctly felt. As soon as I had recovered from the surprise of the moment, I ran down below to state what I saw, and to see if the conductors below had been affected; and just as I entered the gun-room, the purser,

* MS. letter with a drawing, dated July 11, 1828, from the Rev. G. F. Wise, late Vicar of Kingsbridge.

Mr. Rowlett, ran out of his cabin, (along the beam of which a main branch of the conductor passed) and said that he was sure that the lightning had passed down the conductor, for at the moment of the shock he heard a sound like rushing water passing along the beam. Not the slightest ill consequence was experienced; and I cannot refrain from expressing my conviction, that had it not been for the conductor the results would have been of very serious moment.

"This was not the only instance where we consider that the vessel had been saved from being damaged by lightning by Mr. Harris's conductors; and I believe that in saying I had the most perfect confidence in the protection which those conductors afforded us, I express the opinion of every officer and man in the ship.

"Not being sufficiently acquainted with electrical experiments, I cannot remark upon those you have adduced in support of your opinions detrimental to Mr. Harris's conductors.

"I can, therefore, only repeat my conviction that the *Beagle* was struck by lightning in the usual way, and certainly without any *lateral* explosion or other ill effects similar to those you insist on in your *Annals of Electricity*."

35. Now these facts are totally subversive of all Mr. Sturgeon has advanced concerning his destructive lateral explosion in the way of objection to the fixing conductors in ships' masts, and prove in the most conclusive manner the protecting power of such conductors: his statement, therefore, that "destructive lateral discharges will always take place when the vicinal bodies are capacious and near the primitive conductor or to any of its metallic appendages," is clearly fallacious.

36. I is allowed by writers on inductive science, that we wander from the true path of philosophical inquiry, and take up that of assumption and conjecture, directly we cease to verify our principles by an appeal to facts. In order to arrive at a general law of nature, it is requisite to examine carefully a great number of facts bearing directly on the question at issue, and shew, that the principle we assume is common to them all; for if in any case the assumed principle is decidedly negatived, it is at least a powerful exception; and it *may* be sufficient to overturn our whole theory.

If such exceptions are numerous, any theory which cannot include them is decidedly untenable.

It has been well observed by Abercrombie,* that in deducing a general principle, "when the deduction is made from a full examination of *all* the individual cases, and the general

* On the Intellectual Powers.

fact shewn to apply to them all, that is truth ; when is it deduced from a small number of observations and extended to others to which it *does not apply*, this is falsehood."

37. In applying these principles, we find Mr. Sturgeon's assumed lateral explosion decidedly negatived in all the cases just cited, since we do not find any such occur in the passage of heavy discharges of lightning along the masts, &c.; we do not find, as asserted by him, any thing like electrical waves produced by the discharge through a conductor situated close to the magazine. Thus in the case of the *Hyacinth*, No. 1. the copper pump *d e*, fig. 1, was a conductor near the after magazine. Yet the electric shock, in passing down this and through the ship's side, did not cause "intense sparks among the powder barrels, whose metallic linings and hoops reciprocally interchange them."*

38. Again, we do not find in the passage of a dense explosion of lightning that the sailors are necessarily subjected to lateral discharge, since in the case of the *Snake*, it may be observed that a seaman aloft on the cross-trees did not experience *any sensation* whatever, although the top-gallant mast was shivered, and a terrific shock darted from the heel of it to the chain topsail tye. Now if Mr. Sturgeon's views were practically sound, this man ought to have been killed on the spot by a "*lateral discharge*," as he says happened to a seaman called Wilson in the case of the *Rodney*.

39. Mr. Sturgeon, therefore, if he still adheres to his theory, is at last reduced to the necessity of supposing, that his lateral discharge may sometimes occur, and sometimes not, which is manifestly in the teeth of his own hypothesis. This instance just quoted of the little effect experienced by persons in the vicinity of heavy electrical discharges is by no means a solitary one, as the following extract from a letter from Admiral Hawker, with which he favoured me relative to the damage done to the *Mignomne*, very fully shews :—

"The circumstances of the *Mignomne* being struck by lightning were these : she had been on shore, and was going to Port Royal, Jamaica, attended by the *Désirée*; we had a day I think the hottest I ever experienced in the West Indies, without a cloud. After sunset we observed clouds rising up from every part of the horizon with thunder and lightning. I ordered the topsails to be lowered in case of squalls, and we ran down towards Port Royal: about midnight the heavens seemed to be one continued flame, and soon after the main topmast was shattered into probably fifty pieces, scattering

* Sturgeon's *Memoir*, *Annals of Electricity*, &c. vol. iv.

the splinters in all directions; the mainmast was split down to the keelson, and a sulphurous smell came up from the hold, which occasioned some to cry out that the ship was on fire. Two men were killed in the main-top, being burnt black, and having some splinters sticking in them, and a man who was sleeping on the lower deck with his head on a bag (for the ship having been on the rocks for three days there were no hammock-) near the armourer's bench was found dead, with one black speck in his side; *another man sleeping by him was not hurt.*"

40. The number of instances in which dense explosions of lightning have passed very near to persons without causing any serious injury to them is remarkable.

Thus in the case of the Buzzard, No. 4, before mentioned; the explosion at the time of shivering the topmast passed so near to a seaman called Robert Purk, that it actually tore the shirt from his arm: he very kindly shewed me the shirt, and pointed out the place where he was standing. Lieut. Fox, who commanded this vessel, and who was good enough to send me an account of the damage, &c. sustained, says, in allusion to this circumstance, "The lightning took a strip out of the shirt about two inches wide from the shoulder to the wrist without hurting him."

No. 9.—In the instance of the Hawk cutter, lately struck by lightning on the west coast of Erris, and scarcely damaged, it appears that the electric matter in passing down the main hatchway passed between a man and a boy. Neither were hurt; the latter experienced a shock only. It also passed close to another man lying across a hammock about the same spot, who jumped up and thought his neckhandkerchief was on fire; the latter experienced a temporary effect only on his right arm.

41. All these cases evidently show, that no damage occurs from a shock of lightning *out of its direct path*. It may, however, divide in the absence of any good conducting course, and branch out into a variety of other courses (as already observed) and seize either wholly or partially upon bodies which happen to lie in certain points, as clearly shown in all these cases, and in the partial fusion of the leaf-gold given in experiment 2,* of my last communication.

We may also expect to find an *expansive* effect of greater or less force in the vicinity of a discharge of *free electricity* under the form of a dense spark, in a *bad conducting interval*; as observed by Dr. Priestley, "the air being suddenly dis-

* Annals of Electricity, &c. vol. iv. p 492.

placed gives a concussion to all the bodies which happen to be near it."

42. It is clear therefore that in all cases where injury or death has occurred, as in those before given in the *Mignonne*, *Rodney*, &c., it has been the result of the passage of the electric agency, either wholly or partially, through the animal body, and not from the result of any *lateral explosion* of electricity, such as described by Mr. Sturgeon. If, as he says, such explosions in all cases of proximity to the primitive charge necessarily arise, such proximity to the passage of a dense shock of lightning would be in all cases fatal, which is evidently not the case.

43. I have now to consider briefly a few instances of the power of metallic bodies to transmit heavy discharges of lightning.

In the case above quoted of the *Hyacinth*, we observe, as already remarked, that a flash of lightning which shivered the top-mast and top-gallant mast passed over a small iron chain and copper tube without fusing either. A similar result ensued in the second instance of the *Hyacinth* being struck by lightning; also in the case of the *Athol* and *Buzzard*, and *Snake*, and in a great variety of others too numerous to detail here.

In the case of the *Fox*, No. 5, it is seen that the shock of lightning which damaged the mast, was conducted without fusion or damage by sheet copper of $\frac{1}{2}$ nd of an inch in thickness placed in the wake of the gaff. This is conclusive of the fallacy of Mr. Sturgeon's assertion, that any conductor applied to the mast would, under the operation of lightning, be "probably peeled from the wood."

In the case of the *Kingsbridge* spire, No. 6. The lightning which shivered the tower, fell on a cylindrical iron rod of an inch diameter without producing any effect on it.

In the case of the *Rodney*, the flash which set the top on fire and splintered the masts, was conducted by a short copper funnel for top-gallant rigging without fusion.

In the case of the *Beagle*, No. 7, a shock of lightning passed down the conductors without producing any effect on them.

No. 10 A house was struck at Tenterden; the lightning fell on an iron bar three-quarters of an inch square, but produced no effect on it.*

No. 11. A stroke of lightning fell on Mr. West's house, at Philadelphia, having a conductor terminating in a brass rod ten inches long and *a quarter of an inch in diameter*; only a

* Philosophical Transactions.

few inches of the point were melted, but no damage occurred to the building.*

No. 12. On the 19th of April, 1827, one of the large New York packets, whilst in the Gulf Stream, was assailed by two most awful strokes of lightning twice in the same day. The first shock was productive of serious and destructive effects. The second shock fell on a pointed conductor subsequently hoisted to the main-mast head. This conductor consisted of an iron chain, having links of a quarter of an inch thick and two feet in length, and turned into hooks at each end, connected by rings of the same thickness, and one inch annular diameter. This conductor was *attached* to an iron rod placed at the mast head, half an inch thick and four feet long. The explosion fell in a *concentrated* form, and with an awful crash upon this rod. Although the small chain below was disjointed and some of the links fused, yet this pointed iron rod was only fused for a few inches. *The ship in the second case escaped danger.*

Now these are authenticated cases, and there are numerous others which I might adduce, to shew how perfectly *capacious* and *continuous* conductors transmit shocks of lightning.

44. No good instance can be adduced in which conductors of great capacity have been even moderately heated by lightning. I do not admit Mr. Sturgeon's "on dit" respecting the conductor passing through the Nelson Monument in Edinburgh. It is really no evidence whatever on a scientific question. "*It is said*" (observes Mr. Sturgeon) that the lightning rod passing through the Nelson Monument became so hot by lightning that it could not be touched by the hand by *the first person* who visited it afterwards. Allowing a few minutes to have elapsed between the flash and the person entering the monument, the probability would be that the conductor had been made red-hot." This is of the same character with all Mr. Sturgeon's data; it is generally surmise, the *shew* without the reality; it just amounts to nothing.

45. I am aware that it has been also *supposed* that the great conductors of St. Paul's church were heated by lightning, but it is only a *supposition*. The conductors were not examined before the lightning, which was said to have fallen on them, occurred, so that we cannot be certain that the observed appearances were not originally present after the forging of them; it is, besides, very unlikely that a stroke of lightning should have fallen on this building, capable of rendering bars of iron, six inches wide and one inch and a half thick, red-hot, without

* Philosophical Transactions.

destroying the thin copper covering the ball and cross on the dome of the building, and without the crash of the thunder having been heard over the whole city, no mention of which is made; when St. Bride's steeple was struck, the latter was peculiarly remarkable.

46. There is another instance on record of the effects of lightning on an iron rod, in Port Royal, Jamaica, mentioned in the transactions of the Royal Society, the evidence of which seems very incomplete. Two men are said to have perished by lightning near the church wall: that is not improbable: but, on subsequently looking inside the wall, a bar of iron, an inch thick, and a foot in length, was found in many places wasted away to the size of a fine wire. Now it does not appear that this bar was examined *previously* to the occurrence of the lightning; hence we cannot infer that the wasting was produced by the electric fluid; more especially as similar appearances are not uncommon in bars of iron erected in churchyards in this country, and which have evidently resulted from oxidation and time.

47. Seeing then how much evidence we have from actual experience of the protective effect of regular conductors of the *worst* kind, and their power of transmitting dense explosions of lightning, we may reasonably infer that a conductor of copper, equal to a rod of an inch diameter, and *extending the whole length of the mast*, would be proof against any discharge of lightning ever experienced, as, I think, is shewn by the cases in which ships fitted with my conductors have been struck by shocks of lightning without damage.

48. Exceptions, however, have been taken by Mr. Sturgeon to the phenomena described by the officers who either commanded or were in the ships. Thus Captain Turner, in describing the shock of lightning which fell on the Dryad frigate on the coast of Africa, says, that "he saw the lightning on the conductor on the fore-mast, and saw it during another flash run down the mizen-mast; that all the men there heard a loud whizzing noise." Captain Fitzroy and Lieut. Sullivan also mention similar phenomena. Now the exceptions taken are these, viz., that no noise is ever produced by electricity entering a conductor, and that we cannot produce a "running light" upon a conductor carrying an electrical charge.

These exceptions, however, are rather captious objections to forms of expression, than to the facts themselves; it is easy to shew from experience that luminous appearances are often attendant on discharges of both natural and artificial electricity.

Thus in the case of the Hawk, No. 9, the account states that "the vessel was apparently enveloped in a flame of light-

ning;" whilst, in the case of the *Beagle*, Lieut. Sullivan says, "on looking aloft, the ship was apparently in a blaze of fire." In the case of the *Snake*, No. 3, the electric fluid is said to have *descended* with an instantaneous explosion of a vivid purple color.

When H.M.S. *Norge* was struck by lightning in Port Royal harbour, the electric fluid was observed (to use Admiral Rodd's expression) to "absolutely stream down a conductor attached to the mast of H.M.S. *Warrior*," close by.

Such phenomena are besides remarkably close to the results of experiments: thus a heavy shock of electricity, passed over a metallic wire, in a partially exhausted receiver, will exhibit a transiently passing light on its surface.

49. The whizzing noise is quite in accordance with common electrical effects. It invariably occurs when a good conductor receives and disarms an explosion by a pointed extremity. Mr. Sturgeon, however, asserts that "no such noise is ever produced by the *fluid entering* a metallic conductor." This is mere sophistry; let any one attempt to discharge a highly charged battery by an acutely pointed conductor. A great part of the charge will immediately rush through or towards the point with a whizzing noise. Now the stratum of cloud may be either positively or negatively electrified, and whether the one or the other, it is clear that the rush of electricity from a charged surface toward a point, or from a point towards an undercharged surface (according to Franklin's hypothesis) will be always attended by a whizzing noise.

50. The protection which continuous conductors would afford if well and efficiently applied to ships is, I think apparent in all the preceding cases, and when we consider that the masts are themselves conductors of electricity, and that by their position alone they determine the course of the discharge into the body of the hull, it becomes the more requisite to affix to them good conductors, which quickly disperse and reduce the electrical action to a state of quiescence.

We have I think fair evidence of this in the trials hitherto made with the continuous fixed conductors applied to certain ships of the British Navy.

51. These ships have been exposed more or less in all points of the world. Lightning has not fallen upon them *oftener* than other vessels not so fitted; and *and when it has done so no damage* has arisen in any way, or has any destructive lateral effect, such as that contended for by Mr. Sturgeon, taken place. His comparison, therefore, of the effects of lightning on the *Rodney* with the "*probable effects*" (as he terms it) on my

conductors, although he can find no instance of such *probable effects*, is therefore purely hypothetical. If Mr. Sturgeon has no good authenticated fact to oppose to the mass of evidence I have adduced, of what avail is any hypothetical or loose opinion he may find it *convenient* to advance?

52. Before concluding this communication, I cannot refrain from pointing out the apparent inconsistencies of his views on this point. Having described my conductors as dangerous and objectionable in every possible way, as calculated to induce oblique flashes of lightning to strike the ship to the destruction of the sailors' lives, the sails, rigging, &c. &c., he says, sec. 221, on discovering that he could not conveniently apply his own rods above the top-mast head, "*as however every chance of danger to the men and every species of damage to the vessel ought strictly to be avoided*," it still appears desirable to furnish the top-gallant rigging with conductors; and perhaps those which would give the least trouble to the men, would be strips of copper let into grooves of the masts according to the plan proposed by Mr. Harris." Now, I think, it must be clear to any one, that if my system be so objectionable as he would have it believed, on the grounds above stated, it must be equally objectionable on the top-gallant masts; the lives of the sailors are just as much exposed there as at any other point, perhaps more so. Mr. Sturgeon himself admits that two men were killed there in the case of the Rodney. But by his admission above quoted, my method is not objectionable in the top-gallant mast, but is on the contrary calculated to avoid "every species of damage to the vessel and every chance of danger to the men;" if so, it must be equally efficient on the top-mast, lower mast, &c. This sort of traverse sailing, to use a nautical phrase, is not a little amusing, and is, I believe, quite unprecedented in any paper on science.

53. In order that no mistake may arise in respect of what I have advanced relating to lateral explosions, I may in conclusion simply state, that I do not deny the expansive force of a dense electrical explosion, and its destructive effect on *imperfect* and *non-conductors*. I do not deny its effect in causing expansion in the surrounding air, which I rather choose to call with Priestley, "the lateral force of electrical explosions," than a *lateral explosion of electricity*. I do not deny this in the absence of any regular system of conductors, or that the discharge may divide in several directions, and in distributing itself over the hull, may cause dense sparks and other electrical appearances in various parts of the vessel, but which would not appear, if a perfect system of conduction, such as I have proposed, were resorted to.

I do, however, deny the probability of any lateral discharge of electric matter from conducting bodies transmitting an accumulation between oppositely charged surfaces, as assumed by several persons imperfectly acquainted with ordinary electrical action, and lately by Mr. Sturgeon; and, I maintain, that neither artificially, nor in the course of nature, can any instance of such lateral explosion be authenticated.

I am, &c.

W. SNOW HARRIS.

Plymouth, March 14, 1840.

P.S. It has been insisted on by Mr. Sturgeon, that a shock of lightning, descending a continuous conductor on the mast, would magnetize every chronometer in the cabin, &c.—(Memoir, Sect. 207.)

This assumption is completely negatived by the cases above quoted. In fig. 4, an awful discharge descended an iron chain, and yet no magnetic effect was observable on the neighbouring compasses, or on the chronometer in the cabin. It is only in the *absence* of continuous conductors we find such magnetic effects, and even then their occurrence is comparatively rare. Really, Mr. Sturgeon makes so many random assertions, it is almost impossible to attend to them all.

IX.—Mr. STURGEON's *Fifth Letter* to W. SNOW HARRIS, Esq.
F.R.S. on *Marine Lightning Conductors*.

Sir,

When I had finished my last letter to you,* I made up my mind to decline any further notice of your impotent productions in defence of that extraordinary, unnecessarily expensive, and certainly the most unscientific and dangerous plan of marine lightning conductors, that could possibly have been thought of by any one claiming the character of an electrician. But finding, in the preceding article, a few descriptions of the effects of lightning on shipping, which, if correct, can hardly fail to be interesting to the electrician, I have not hesitated to give them a place in these Annals; and as you have laid considerable stress on these events, as sure indications of the infallibility of your electrical philosophy and lightning conductors, I have again ventured a few remarks, not with any hope of convincing you of your errors, or rather of your acknowledging them, but to shew you that a very different explanation to that which you have attempted, would look quite as

* Annals of Electricity, &c., vol. iv. p. 496—500.

plausible to account for some of the effects produced by lightning on those vessels; and in order to facilitate the comparison, I will, as on former occasions, follow the numerical order of your own paragraphs.

I think that I may very justly remark, as an introductory proposition, not difficult of demonstration, that, *if there be no motive beyond the propagation of truth*, one of the causes of your committing so many errors in calculating on the effects of lightning, is simply by your imagining that *all discharges of lightning are alike powerful*.

In paragraph 26, you have, no doubt, given a very exact account of the dimensions of the chain topsail-sheet, and of the copper pipe of Hearle's pump. Then, because each link of the former consisted of two sides, it was *virtually* composed of two iron rods, each of which was half an inch diameter from one end to the other; and as the latter "was three inches in diameter," the copper sheet of which it was made was uniformly nine inches broad. Hence the topsail-sheet and Hearle's pump were no mean conductors, even compared with your own; for the copper pipe of the pump was nearly of the same transverse dimensions as the *mean* of yours on the *lower masts*; of much greater transverse dimensions than yours on the *top-masts*; twice the transverse dimensions of yours on the *top-gallant masts*; and more than twice the transverse dimensions of your royal conductors. Moreover, since "the effects on the fore-mast were very similar" to those produced on the main-mast, we are led to believe that each mast received only one half of the *main stroke*. And again, by considering also, that *each mast alone* would carry some portion of the lightning to the sea, then, taking all these circumstances into account, and also the probability of this flash being very far from the most formidable that occurs, I cannot see that this case is any proof either of the efficiency or inefficiency of your conductors, nor can I see what advantage you could think of gaining in defence of them, by bringing such a circumstance forward, in which the conductors which were not injured were, in some parts of the circuit, more than four times the dimensions of yours.

Paragraph 28 may possibly be a very correct account of the effects of a flash of lightning on the Athol; and so far as *description* is concerned, it is an interesting paragraph. But I do not agree to what you say in paragraph 29, viz., that "these effects are similar to the former, and shew the protection afforded by *the* chains, and their power of conducting *heavy* discharges of lightning;" because, if I did, I should have to acknowledge that all "*heavy* discharges of lightning"

were of precisely the same power ; which would be the very opposite to the views which I take of these operations of nature ; and, what would be worse still, if possible, I should have to acknowledge that *half a flash of lightning* ought to produce precisely the same effect as the whole flash would do. This, as I first observed, is one of the rocks on which you so frequently founder.

The effects of lightning on H.M.S. Snake, as described in paragraph 30, are also very curious and interesting ; more especially if Captain Milne's account of the route of the electric fluid be correct. Perhaps you can explain why the electric fluid jumped from "within eight feet of the deck" to "the saddle of the main boom;" and by what route, from "the saddle of the main boom one portion passed out of the quarter-deck port to the sea."

Paragraph 31 is an obvious indication of the limited views which you have of lightning, and of the correctness of my first remark in this letter, viz., that you consider all flashes of lightning to be of the same power ; and that the power of half a flash is equal to that of the whole flash. This inference, you will find, is clear enough, when you compare the description of the two events alluded to, in this paragraph, in which you say the Buzzard "lost her top-gallant and top-mast, under precisely the same circumstances as those of the Hyacinth;" for it is obvious that the Hyacinth's main-mast was struck by only *half* of the original flash ; whereas the mast of the Buzzard was struck by the *whole* flash.

The only inference which, in a philosophical point of view, can be drawn from your 32d paragraph, is that the Fox revenue cutter was struck by a comparatively feeble stroke of lightning ; although, from the manner in which you appear to apply the case, that paragraph becomes demonstrative of your belief that all flashes of lightning are of precisely the same power.

You seem to be very desirous to shew your readers, that "the copper usually placed about a cutter's mast is not the $\frac{1}{16}$ th part of an inch in thickness;" and that "in this case it remained perfect." Now it strikes me that some of your readers will ask, why you did not give them the *other* dimensions of the copper ? A candid, scientific reasoner would not have left them in doubt on a point of such essential importance in varying the effects of a flash of lightning on the metal. Even you own tinsel experiments (figs. 4, 5, and 6, plate x., vol. iv.) must have taught you that a broad strip of gold leaf may remain perfect, though it be traversed by an electric discharge which would destroy a *narrow* strip of the same thickness ;

and that a still more powerful discharge might have destroyed both of them. Whether or not there be a fatality attending your philosophy, over which you have no control, is not for me to determine; but there seems something curious enough in placing the power of a flash of lightning, which just "furrowed" the mast of the Fox cutter, on a par with the power of that flash which produced such tremendous havoc on board the Rodney, or with that which destroyed "both the fore and main-top masts and top-gallant masts" of the Hyacinth!!!

With respect to the effects of lightning on "the spire of a church at Kingsbridge," as described in paragraph 33, I can have no doubt of your having a very correct account from the Rev. G. F. Wyse; and I have only to request the same favour on your part, whilst you read an account of another flash of lightning, the effects of which were also described by a reverend gentleman.

On the 28th of April last, the bishop of Nova Scotia, in company with the Rev. Mr. Cardwell, called at this Institution, and entered into a conversation with me on the subject of atmospheric electricity. The bishop, who is well acquainted with electricity, described several curious effects of lightning which had come under his own observation. On one occasion, where lightning had struck a conductor which was fastened close to the wall of a building, a portion of the brick-work behind the conductor was crushed to powder, and a deep furrow, parallel to the conductor, was made in the wall from top to bottom.

At another time, the bishop saw a flash of lightning ascend from the earth to the clouds, lifting up, in its passage, a great quantity of the soil and other earthy matter from the surface of the ground to a great height.

These two remarkable circumstances being described by an eye-witness of such high authority as the bishop of Nova Scotia, may justly be regarded as exceedingly interesting events in the history of atmospheric electricity. The furrow being made in the wall behind the conductor, is an excellent contrast to the effects of lightning on the spire of the church at Kingsbridge, and shews that, although the iron rod was sufficient to conduct a flash of lightning of a certain force with safety; yet, the lateral forces of a still more powerful flash might possibly not only furrow, but totally destroy the spire to which it is attached.

Your 34th paragraph is merely a copy of Lieut. Sullivan's letter already printed in these *Annals*,* and, therefore, I

* Vol. iv. p. 327.

can have nothing to remark upon it in this place, excepting that I may be permitted to say, that it is an exceedingly interesting description of the appearances on board the *Beagle* at the time she was *supposed* to be struck by lightning, and, unquestionably, is the best description of those appearances that has hitherto been given; and so very different to that given by Captain Fitz-Roy,† that they scarcely appear to relate to the same event. I do not see, however, that even Lieut. Sullivan's description can be considered to be "totally subversive of all" that I have "advanced concerning destructive lateral explosions." That officer candidly acknowledges that he is not sufficiently acquainted with electrical experiments to offer any remarks on those which I have adduced. Now, sir, had you also acknowledged that you were not sufficiently acquainted with atmospheric electricity to offer any remarks on those phenomena which I have described in my fourth memoir, I should have considered that you also were enjoying the same honourable feelings.

The philosophy of paragraph 36 is exceedingly good; and Abercrombie's doctrine is perfectly applicable in the present instance; for if your ideas of atmospheric electricity had been formed from a sufficient number of facts collected from your own observations, they would have been much more comprehensive than at present; but from a want of that experience so essential to the formation of a sound judgment of all the variety of atmospheric electrical operations, and, to distinguish one class of them from another, your views of this branch of the science are necessarily very limited; and, having confessed that you have no acquaintance whatever with atmospheric electrical *waves*, it is not to be expected that you can have any knowledge of the splendid phenomena which they produce on high elevated conductors. Hence it is that you have fallen into error by supposing that the *Dryad* and *Beagle* were struck by lightning; though to a person well experienced in electrical kite experiments, it becomes obvious enough that the lightning never struck either vessel in the cases alluded to. And a person only *moderately* acquainted with electro-magnetism would know well that the *primitive* discharge never traversed the *Beagle's* main-mast conductor on that occasion.

Whilst writing my fourth memoir, I was particularly careful in advancing no experimental facts but those with which I was quite familiar: hence it was that I described no other electric kite experiments than those I had myself made, nor any of those splendid phenomena witnessed by other experimenters,

* Vol. iv. p. 327.

whilst exploring the atmosphere in a similar manner : hoping, from the confidence I then placed in your candor, that your desire to promote truth would have induced you to allude to them yourself ; especially those phenomena seen by M. de Romas at his kite-string. Those phenomena were of a similar character to some of those which I have described,* and were obtained under similar circumstances ; and had you brought them forward in your papers, as I expected you would have done, they would have given a fair opportunity to your readers to form a just comparison between them and the phenomena seen on board the *Beagle* and the *Dryad*.†

Your reasoning in paragraph 37 is curious enough, implying that, because the *Hyacinth* was not *blown up*, there could be no lateral sparks!!!

Paragraph 38 is a twin-sister to its predecessor, and implies that, because the man in the cross-trees was not killed, there could be no lateral discharge!! Why ought the man to have been killed? That he had a very narrow escape, no one will deny, when the circumstances are properly understood. "The top-gallant mast was shivered, and a *terrific shock* darted from the heel of it to the chain topsail tye." Now this "*terrific shock*" was one of those cases in which that kind of lateral force is produced which Priestly calls the *lateral explosion*, and which you have been forced to acknowledge in paragraph 53. When you were writing that *confessional* paragraph, in which you say, "I do not deny the expansive force of a *dense electrical explosion*, and its destructive effects on *imperfect*, and *non-conductors*," I suppose you had forgotten the "seaman aloft on the cross-trees," close to the "*terrific shock*," or *dense electrical explosion*!!"

With respect to Wilson, who was killed in the *Rodney*, since there were no marks to be found either on his body or his clothes, it is fair to infer that he suffered from the lateral forces, and not by the primitive discharge which killed his shipmate.

In paragraph 39, you again attempt to lead your readers astray by telling them that, by my theory, as you are pleased to call it, a "lateral discharge may sometimes occur, and sometimes not." If, instead of insulting your readers by thus attempting to impose upon their credulity, you had referred them to paragraph 203 of my fourth memoir, page 176, vol. iv. of these Annals, the only inference which they would have drawn would have been the following:—"As the extent of electro-displacement, in vicinal bodies, depends upon the mag-

* See my Fourth Memoir.

† M. de Romas's experiments are described in the appendix to this letter.

nitude and intensity of the primitive discharge or *main stroke*," and as those conditions may probably vary with almost every flash of lightning, some flashes may be of such feeble powers as to produce no very formidable *lateral* effects; although others may be sufficiently powerful to exert lateral forces productive of the most serious consequences."

Admiral Hawker's account of the effects of lightning in the *Mignomne*, is very interesting. As "the main-mast was split down to the keelson," there can be no question about the principal charge being transmitted in that direction; and, consequently, the man who was killed "near the armourer's bench," suffered either by the *lateral* force of the main stroke, or by so small a portion of the latter, as to produce no serious *lateral* effects on his shipmate who was "sleeping beside him." Moreover, the two men who were killed in the main-top, "being burnt black," shews pretty clearly that they suffered from a very superior force to that which killed the man below, whose external injuries were only "one black speck in his side." The main top-mast being shattered to pieces, and the splinters being scattered "in all directions," is another instance of the formidableness of *lateral explosions*.

I am much obliged to Robert Purk for shewing you his shirt; for as the man was not injured, it is pretty clear that neither he nor his shirt were struck by the lightning; and that it was the *lateral* force which "took a strip out of his shirt about two inches wide from the shoulder to the wrist without hurting him." The most probable *immediate* cause of the shirt being torn was a sudden distention of the air within the sleeve. See paragraph 40.

The cases which occurred in the *Hawke* cutter were obviously the effects of *lateral electric forces*. The "boy experienced a shock only," not being hit by the lightning. Neither did the lightning *strike*, but only "*passed close* to another man," who only experienced a temporary effect in his left arm." I am not certain that you could have produced better data than these to prove the injurious effects of lateral explosions.

Your mode of accommodating your philosophy to facts is truly curious and ingenious, and as nearly *opposite* to that exercised by profound reasoners as any one could be led to expect. In paragraph 41, you say "that no damage occurs from a shock of lightning *out of its direct path*." This beautiful philosophical inference is a master-piece of its kind, emanating, as it obviously does, from the fact which you described the moment before, in which you say, "that the electric matter, in passing down the main hatchway, passed *between* a man and a boy;" and that these persons, who were "out of the *direct path*" of the lightning, were both affected by it.

From the above specimen of your philosophy I pass on to your case No. 12, in which I am glad to find that you view the effects of lightning on the New York packet more seriously than in paragraph 7.* The best account of the damage done in this vessel is that given by Mr. Rich.† The spindle was fused for several inches of its length, and the chain "conductor" was literally torn to pieces and scattered to the winds."

With respect to the Nelson Monument at Edinburgh, I have nothing to add to the fact which I have previously stated; excepting that I may here remark, that your attempt to place that fact or any other which bears on this important topic, in the back-ground, indicates a desire to evade those cases which ought to be particularly attended to. It is extreme cases of the effects of lightning that ought to be guarded against; and if those cases be not provided for, with regard both to the dimensions and position of the metal, no conductor can give the necessary protection.

Your obvious *intentional* attempt, in paragraph 48, to pervert the meaning of some of those points on which I have touched in my fourth memoir, tends to excite a strong suspicion that the whole of your perversions have emanated from some *unaccountable motive*, of a very different nature to that which would have been expected from a person of your pretensions. How dared you attempt to make it appear that I have said "that we cannot produce a running light upon a conductor carrying an electric discharge"? How dared you venture to palm upon your readers such a palpable barefaced untruth? My language on this topic is the following:—"We cannot produce any thing like a *running light* when the conductors are sufficiently good and capacious to conceal the motion of the fluid; though such a phenomenon may easily be produced by the employment of inferior conductors.‡

With respect to the appearances on board the Dryad and Beagle, I have already expressed my opinion pretty clearly; and I have not met with any statement of the facts which has the least tendency to alter that opinion.

That the Hawk cutter should appear as if "enveloped in a flame of lightning," from the luminous effects of the flash which struck her, appears probable enough, although it is certain that nothing of the kind occurred; for "the electric matter in passing down the main hatchway, passed between a man and a boy," and therefore could not envelope the vessel. The flash, *prior* to its striking an object, produces the greatest lu-

* Annals of Electricity, vol. iv. p. 487.

† Ibid. p. 372.

‡ Annals of Electricity, &c. vol. iv. p. 186.

minous effects ; and lightning passing *near* to either a ship or a house would produce similar luminous effects to those which appeared to the people on board the *Hawk* and the *Beagle*.

Dr. Franklin's opinion on these luminous effects of lightning will be seen in the Appendix to this letter.

Another instance of those mean attempts to pervert the meaning of certain topics of my fourth memoir appears in paragraph 52, in which you take to yourself a great deal of credit, by conveying to your readers as profound a falsehood as ever proceeded from man : stating, as you do, that I have admitted that your method of protecting the masts of ships is not objectionable. This paltry manœuvre strengthens my former suspicions, and gives a very sable coloring to the *motives* from which emanate such unjust aspersions. If you had regarded truth, and the just interests of science, whilst quoting my remarks on conductors for top-gallant masts, you would have directed your readers to paragraphs 221 and 222 of my fourth memoir,* in which I have stated, that, "instead of only *one* strip (of copper) to each mast, I should propose *three* in each, at equal distancee from each other ; which, by having an exposure of metal on every side, would be a greater security to the mast than by having one strip only. And that four cylindrical copper rods, or four flexible metallic ropes, stretched from the cross-trees to the truck, parallel to the top-gallant shrouds, would afford a much better protection to the top-gallant rigging than conductors let into the masts."

It is not for me to judge of the opinions which other readers form of your philosophy, but to me you seem to have been led into the most extraordinary inaccuracies in many parts of your defence of your lightning conductors ; and, perhaps, in none more so, than in the postscript to the preceding paper, in which you appear to be determined, either to mislead your readers, or, to shew your almost entire ignorance of electro-magnetic action. Permit me to ask you a few questions on this subject. Do you wish me to understand, that you, a Fellow of the Royal Society, are totally ignorant of Sir Humphrey Davy's experiments, by which that philosopher first magnetized steel needles by transmitting electric discharges from a battery of jars, through a vicinal conducting wire ? Do you wish me to understand that you, a Fellow of the Royal Society, with the pretensions of an electro-magnetist, never repeated those beautiful experiments ? Do you wish me to understand that you, a Fellow of the Royal Society, who, as an inventor of a marine lightning conductor ought to be a profound elec-

* *Annals of Electricity, &c.*, vol. iv. page 185.

trician and electro-magnetist, that *you* who are pretending to protect the British Navy and our brave tars from the effects of lightning,—that you, on whose judgment such mighty interests are to be at stake,—are entirely ignorant of the laws of electro-magnetism? If you are not entirely ignorant of the magnetic action of electric currents traversing good conductors, how dared you venture to say, that “it is only in the *absence* of continuous conductors we find such magnetic effects?” If you are not entirely ignorant of such magnetic action, how dared you venture to stain the pages of British science, to insult the dignity of the Royal Society, and, above all, to deceive the Lords Commissioners of the Admiralty, and the whole British Navy, by propagating such a palpable falsehood? Will you acknowledge that you are ignorant of the magnetic action of lightning whilst traversing good conductors; or will you have to submit to the degrading position of having *wilfully* concealed that most important fact, to guard against which is one of the most essential considerations in the erection of marine lightning conductors?

I have paid considerable attention to the statements of professors Faraday and Wheatstone in the “Report of the Committee appointed by the Admiralty*,” but I have not been able to discover any facts, in those statements, which have the least tendency to alter my opinion of your plan of conductors, and certainly none whatever tending to invalidate any part of my fourth memoir. By quoting M. De Romas’s kite experiments, professor Wheatstone has shewn, pretty clearly, that electrical discharges such as appeared on the conductors of the Dryad and the Beagle, are no sure indications of those vessels being struck by lightning; for, as will be seen in the appendix to this letter, no lightning was present at the time that the French philosopher was conducting his kite experiments, in which he saw “sheets of fire 9 or 10 feet long and an inch broad, which made as much or more noise than the reports of a pistol.” Perhaps the most remarkable feature in the Report of the Committee, is the total absence of any consideration respecting the magnetic action which lightning would produce on chronometers, compass needles, &c. whilst traversing vicinal conductors; the consequences of which, in misleading the mariner, might be more fatal than those from the direct action of the lightning itself.

I remain, Sir, yours &c,

W. STURGEON.

Victoria Gallery, of Practical Science, Manchester,
June 22nd, 1840.

* Page 1 of this volume.

Appendix to Mr. STURGEON'S Letter.

Amongst Dr. Franklin's remarks on Mr. William Maine's Account of the effects of lightning on his Lightning Rod, we find the following :

"It is said that *the house was filled with its flash*. Expressions like this are common in accounts of the effects of lightning, from which we are apt to understand that the lightning filled the house. Our language indeed seems to want a word to express the *light* of lightning as distinct from the lightning itself. When a tree on a hill is struck by it, the lightning of that stroke exists only in a narrow vein between the cloud and tree, but its light fills a vast space many miles round ; and people at the greatest distance from it are apt to say, "the lightning came into our rooms through our windows." As it is in itself extremely bright, it cannot, when so near as to strike a house, fail illuminating highly every room in it through the windows ; and this I suppose to have been the case at Mr. Maine's ; and that, except in and near the hearth, from the causes above-mentioned, it was not in any other part of the house ; *the flash* meaning no more than *the light* of the lightning.—It is for want of considering this difference, that people suppose there is a kind of lightning not attended with thunder. In fact there is probably a loud explosion accompanying every flash of lightning, and at the same instant ;—but as sound travels slower than light, we often hear the sound some seconds of time after having seen the light ; and as sound does not travel so far as light, we sometimes see the light at a distance too great to hear the sound."—*Franklin's Letters*.

M. DE ROMAS'S Kite Experiment.

"The greatest quantity of electricity that was ever brought from the clouds, by any apparatus prepared for that purpose was by M. De Romas, assessor to the presideal of Nerac. This gentleman was the first who made use of a wire interwoven in the hempen cord of an electrical kite, which he made seven feet and a half high, and three feet wide, so as to have eighteen square feet of surface. This cord was found to conduct the electricity of the clouds more powerfully than an hempen cord would do, even though it was wetted ; and, being terminated by a cord of dry silk, it enabled the observer (by a proper management of his apparatus) to make whatever experiments he thought proper, without danger to himself.

"By the help of this kite, on the 7th of June, 1753, about

one in the afternoon, when it was raised 550 feet from the ground, and had taken 780 feet of string, making an angle of near forty-five degrees with the horizon ; he drew sparks from his conductor three inches long and a quarter of an inch thick, the snapping of which was heard about 200 paces. Whilst he was taking these sparks, he felt, as it were, a cobweb on his face, though he was above three feet from the string of the kite ; after which he did not think it safe to stand so near, and called aloud to all the company to retire, as he did himself about two feet.

“ Thinking himself now secure enough, and not being incommoded by any body very near him, he took notice of what passed among the clouds which were immediately over the kite ; but could perceive no lightning either there or any where else, nor scarce the least noise of thunder, and there was no rain at all. The wind was West, and pretty strong, which raised the kite 100 feet higher, at least, than in the other experiments.

“ Afterwards casting his eyes on the tin tube, which was fastened to the string of the kite, and about three feet from the ground, he saw three straws, one of which was about one foot long, a second four or five inches, and a third three or four inches, all standing erect, and performing a circular dance, like puppets, under the tin tube, without touching one another.

“ This little spectacle, which much delighted several of the company, lasted about a quarter of an hour ; after which, some drops of rain falling, he again perceived the sensation of the cobweb on his face, and at the same time heard a continual rustling noise, like that of a small forge bellows. This was a farther warning of the increase of electricity ; and from the first instant that M. De Romas perceived the dancing straws, he thought it not advisable to take any more sparks even with all his precautions ; and he again entreated the company to spread themselves to a still greater distance.

“ Immediately after this came on the last act of the entertainment, which M. De Romas acknowledged made him tremble. The longest straw was attracted by the tin tube, upon which followed three explosions, the noise of which greatly resembled that of thunder. Some of the company compared it to the explosion of rockets, and others to the violent crashing of large earthen jars against a pavement. It is certain that it was heard into the heart of the city, notwithstanding the various noises there.

“ The fire that was seen at the instant of the explosion had the shape of a spindle eight inches long and five lines in diameter. But the most astonishing and diverting circum-

stance was produced by the straw, which had occasioned the explosion, following the string of the kite. Some of the company saw it at 45 or 50 fathoms distance, attracted and repelled alternately, with this remarkable circumstance, that every time it was attracted by the string, flashes of fire were seen, and cracks were heard, though not so loud as at the time of the former explosion.

"It is remarkable, that, from the time of the explosion to the end of the experiments, no lightning at all was seen, nor scarce any thunder heard. A smell of sulphur was perceived, much like that of the luminous electric effluvia issuing out of the end of an electrified bar of metal. Round the string appeared a luminous cylinder of light, three or four inches in diameter; and this being in the day-time, M. De Romas did not question but that, if it had been in the night, that electric atmosphere would have appeared to be four or five feet in diameter. Lastly, after the experiments were over, a hole was discovered in the ground, perpendicularly under the tin tube, an inch deep, and half an inch wide, which was probably made by the large flashes that accompanied the explosions.

"An end was put to these remarkable experiments by the falling of the kite, the wind being shifted into the east, and rain mixed with hail coming on in great plenty. Whilst the kite was falling, the string came foul of a penthouse; and it was no sooner disengaged, that the person who held it felt such a stroke in his hands, and such a commotion through his whole body, as obliged him instantly to let it go; and the string, falling on the feet of some other persons, gave them a shock also, though much more tolerable*.

"The quantity of electric matter brought by this kite from the clouds at another time is really astonishing. On the 26th of August, 1756, the streams of fire issuing from it were observed to be an inch thick, and 10 feet long. This amazing flash of lightning, the effect of which on buildings or animal bodies, would perhaps have been equally destructive with any that are mentioned in history, was safely conducted by the cord of the kite to a non-electric body placed near it, and the report was equal to that of a pistol.

"M. Romas had the curiosity to place a pigeon in a cage of glass, in a little edifice, which he had purposely placed, so as that it should be demolished by the lightning brought down by his kite. The edifice was, accordingly, shattered to pieces, but the cage and the pigeon were not struck†.

* *Gents. Mag.* for August 1756, p. 378.

† *Nollet's Letters*, vol. ii. p. 239.

"The Abbé Nollet, who gives this account, adds, that if a stroke of this kind had gone through the body of M. De Romas, the unfortunate professor Richman had not probably been the only martyr to electricity, and advises, that great caution be used in conducting such dangerous experiments.*

"When we consider how many severe shocks the most cautious and judicious electricians often receive through inadvertence, we shall not be surprised that when philosophers first began to collect and make experiments upon real lightning, it should sometimes have proved a little untractable in their hands, and that they were obliged to give one another frequent cautions how to proceed with it.

"The Abbé Nollet, as early as the 1752, advises that these experiments be made with circumspection; as he had been informed, by letters from Florence and Bologna, that those who had made them there had had their curiosity more than satisfied by the violent shocks they had sustained in drawing sparks from an iron bar electrified by thunder. One of his correspondents informed him, that once, as he was endeavouring to fasten a small chain, with a copper ball at one of its extremities, to a great chain, which communicated with the bar at the top of the building (in order to draw off the electric sparks by means of the oscillations of this ball) there came a flash of lightning, which he did not see, but which affected the chain with a noise like that of wild fire. At that instant, the electricity communicated itself to the chain of the copper ball, and gave the observer so violent a commotion, that the ball fell out of his hands, and he was struck backwards four to five paces. He had never been so much shocked by the experiment of Leyden."†—*Priestley's History of Electricity*.

X.—Description of a Cast Iron Voltaic Battery, and an Account of some of its Performances. By William Sturgeon.

Having given a notice in the last Number, that I would describe this battery in the present one, I now proceed to do so.

The battery consists of ten cast-iron cylindrical vessels, and the same number of cylinders of amalgamated rolled zinc with diluted sulphuric acid. The cast-iron vessels are 8 inches high and $3\frac{1}{2}$ inches diameter. The zinc cylinders are the same height as the iron ones, and about 2 inches diameter, and open throughout. The iron and zinc cylinders are attached, in pairs, to each other, by means of a stout copper wire, as seen

* Phil. Trans. vol. lii. pt. i. p. 342.

† Phil. Trans. vol. xlviii. pt. i. p. 205.

in fig. 7, plate 1. The zinc of one pair is placed in the iron of the next, and so on throughout the series; contact being prevented by disks of millboard placed in the bottom parts of the iron vessels.

As it is my intention to embody a series of experiments made with this battery, with a number of others, in a paper which will appear in the August number of this work, I will merely notice a few, in this place, which will give a tolerably good idea of its powers.

With ten pairs in series, I have usually obtained 14 cubic inches per minute of the mixed gases from the decomposition of water, and $10\frac{1}{2}$ cubic inches when the battery had been in action an hour and a half. But on the 20th inst. I obtained 20 cubic inches per minute, and this day I obtained 22 cubic inches per minute with the same arrangement; fused 1 inch of copper wire of $\frac{1}{15}$ of an inch diameter; four inches was kept white hot; and 18 inches of the same wire was kept red hot in broad daylight.

Eight inches of watch main springs was kept red hot, and 2 inches white hot, for several successive minutes.

We now employ this battery daily at this Institution.

W. STURGEON.

Royal Victoria Gallery,
For the Encouragement of Practical Science, Manchester,
June 22, 1840.

XI.—MISCELLANEOUS ARTICLES.

Curious Remarks on the Wreck of the "Royal George."

At a recent meeting of the Geological Society, there were read, 'Remarks on the structure of the Royal George, and on the condition of the timber and other materials brought up during the operations of Colonel Pasley in 1839,' by Mr. Creuze. The Royal George was the first ship built on the improved dimensions recommended in consequence of an inquiry into the superior sailing qualities of the vessels of war in the French and Spanish services. She was commenced at Woolwich in 1746, launched in 1756, and, after bearing a very high character as a ship of war for twenty-six years, was accidentally sunk at Spithead on the 29th of August, 1782. From an examination of the various portions of the wreck recovered by the operations of Colonel Pasley, Mr. Creuze states that the great agent of the work of destruction, during the fifty-seven years since the loss of the Royal George, has been "the worm," which has gradually, by its innumerable perforations on every exposed portion of the wood work, reduced it to such

a state as to enable the constant wash of the tides to abrade it layer by layer. The portion of the ship which has thus been removed is considered to be the whole of the upper part, including the topsides above the line of the middle-deck ports. The portions of the recovered timbers which had been buried in the mud were perfectly sound; and Mr. Creuze is of opinion that the bottom of the ship, which is thus protected, and too deeply inhumed to be affected by the explosion, will last for ages. Some portions of the copper have undergone so little change, that several whole sheets average the same weight per square foot as those now used in the royal navy; and this state of preservation, Mr. Creuze believes, may be accounted for on the principle applied by Sir Humphrey Davy to the protection of the sheathing of ships. The cast-iron guns which have been recovered were so much softened as to be easily abraded by the finger-nail to the depth of one-sixteenth and one-eighth of an inch, but they gradually hardened on exposure to the atmosphere. The brass guns are as sharp in their ornamental castings, and apparently as sound, as at their first immersion. A piece of two-and-a-half inch cable-layed cordage, made from a specimen of tarred rope (possibly part of the ship's old junk for sea-store, or of one of the cables used in an attempt to weigh her soon after she sunk), was found to bear 21cwt. 3qrs. 7lbs.; while a similar cable, made from yarn spun in 1830, bore only 20cwt. 1qr. 7lbs. Mr. Creuze then stated some peculiarities in the structure of the Royal George, and concluded with a descriptive catalogue of a series of specimens which accompanied Mr. Creuze's paper.

Further Particulars respecting the Royal George.

Colonel Pasley began his proceedings for the removal of the wreck of the Royal George on the 1st of this month, but up to this day (Monday) nothing very remarkable was effected. Two guns, the rudder, and a considerable quantity of timber were recovered; but as these were merely those fragments of last year's work which the inclemency of the season prevented the engineers from picking up, no serious measures were deemed necessary till yesterday. At eight o'clock in the morning, the red flags at Spithead announced that a great explosion was to be attempted: and at eleven o'clock one of those huge cylinders, which have already been described, and filled with 2,116lbs. of gunpowder, was lowered to the bottom. One of Colonel Pasley's divers (George Hall), who has acquired great expertness in these operations, descended his rope ladder a little in advance of the cylinder, and succeeded in fixing it

securely to one of the lower gudgeons or braces on the rudder-post, within six or eight feet of the keel. The diver having remounted, and the vessels being withdrawn to a safe distance, the enormous charge was ignited by means of the voltaic apparatus. Within less than two seconds after the shock was felt, the sea rose over the spot to the height of about fifteen feet, or not quite half so high as it did on the occasion of the great explosions last year; a difference ascribable, probably, to the cylinder on the present occasion having been placed under the hull instead of alongside it. The commotion in the water, however, was so great, as to cause the lumps and lighters to pitch and roll at a great rate. The whole surface of the sea, for several hundred yards round, was presently covered with dead fish and small fragments of the cylinder. Amongst these were innumerable tallow candles, and a mass of butter a foot and a half in length, evidently driven up from the purser's store-room. As soon as the vast commotion in the water had subsided, and the boats had returned from the universal scramble for the candles and dead fish, the diver proceeded again to the bottom, and soon reported that the whole stern of the ship had been driven to pieces, and that, so far as he could ascertain, there was now a free and wide channel directly fore and aft the ship, from stem to stern, through which both the flood and ebb tides will rush; and thus the mud with which the hull of the *Royal George* has been silted for half a century will be washed out, and the way cleared for Colonel Pasley's further operations. From the auspicious manner, indeed, in which he has commenced, we may safely predict his final success; and we confidently trust that, before the season closes, Spithead will be cleared of this grievous and long-standing drawback to its efficiency as a roadstead for line-of-battle ships.

Further Particulars.

The operations have continued daily with great activity and success, two divers being employed every slack tide in slinging the fragments of the wreck. The stern-post has been got up, broken into three pieces by the great explosion of the 11th instant, together with a large fragment of dead-wood, that stood over the keel, and was also connected with the stern-post. A very curious mass, consisting of part of the lower deck, with a portion of beam, and two knees below the deck-plank, and a rider or upright knee above it, together with part of a port, and the remaining, both of the inside and outside, planking, on each side of a fragment of timber, may now be

seen in the Dock Yard. A very large cable has also been got up, measuring twenty-four inches round, and about ninety fathoms in length, which was generally very sound, but has been broken into several pieces, so that the diver had to descend repeatedly for two or three days before he slung the whole of it. All is clear now above the orlop deck, except some beams of the lower deck, which still remain. This day (Saturday) red flags were hoisted on board the two lumps, at ten o'clock, as a signal that two explosions, of 250lbs. of powder each, would take place at the next slack tide, and two divers were sent down to make preparations for placing the two charges, one under the main hatch of the orlop deck, the other near the bread room. Lieut. Symonds, the executive engineer, who made all the arrangements on this occasion, as well as for the great explosion of the 11th, then sent down the charges with the divers, and having removed the lumps to a little distance, he posted himself at one voltaic battery, whilst Sergeant-Major Jones had charge of the other. Colonel Pasley then gave the word to fire, but only one explosion took place, which was effected by four cells of Professor Daniell's battery, at the distance of 240 feet. This produced the usual effect of a great commotion in the water, in the form of an inverted bowl, spreading to a considerable distance, but not rising to any great height; several seconds elapsed, after a sharp shock was felt, before this agitation of the water took place. The second explosion, which was to have been fired by means of Mr. Alfred Smee's new voltaic battery, did not take place on completing the circuit; but Sergeant-Major Jones, feeling the shock of the other explosion, believed it to be his own, for he completed the voltaic circuit, on first receiving the order to fire. Being ordered to complete the circuit a second time, he did so; and on keeping up the contact for about four seconds, the explosion was effected at the distance of 460 feet. After these explosions, which were witnessed by Admirals Sir Edward Codrington, and Bouverie, Major-General Sir Hercules Pakenham, and a number of officers of both services, and numerous other spectators, the divers repeatedly went down again, and lashed large pieces of timber, amongst which were the parts of a lower deck beam. A human skull, with teeth, was also brought up from the after part of the wreck, which Colonel Pasley has declared his intention to bury in Kingston churchyard, together with such other remains of skeletons as may be obtained hereafter.

*Operations against the Wreck of the "Royal George,"
and proposed Great Explosion.*

The mud accumulated in the hold of the wreck having proved troublesome to the divers, a number of small charges of 47lb. and of 260lb. of gunpowder have been fired against the wreck within the last fortnight, and the removal of the fragments has proceeded with great activity; but it now appears necessary to have recourse to another great explosion of about 2,160lb. of powder, to be placed in a wooden cylinder made in Chatham dockyard, which having been coated with a waterproof composition, and sunk in fifteen fathoms at Spithead, was declared to be perfectly water tight. Colonel Pasley has declared his intention of firing this great charge at about a quarter before two o'clock on the afternoon of Monday, the 22nd instant, when the neap tides and long slack water will favour the operation. Red flags will be hoisted on board the Success frigate hulk, and the two lumps or mooring lighters at Spithead, several hours before the explosion on the day above-mentioned. Should a violent gale of wind occasion such a swell as to prevent the operation on the 22nd, it will be postponed till the 23rd or 24th, and each day of delay will cause the explosion to take place about three quarters of an hour later than the time before mentioned.

Sketch of the Life of the late Lieut. Bell.

"John Bell was the eldest son of a hat-manufacturer of respectability and considerable property, residing in Carlisle, and was born on the 1st of March, 1747. Until he attained the age of eighteen, he assisted in the management of his father's business; indeed, from his parent having engaged in scientific pursuits, and more particularly in the vain endeavor to discover the longitude, the duties of the business almost wholly devolved on the subject of this sketch. In the year 1765, Sergeant Harding, of the Royal Artillery, who was familiarly known to the family, being at Carlisle recruiting, young Bell was induced to enter into the service of his country, and after having received the usual drilling at Woolwich, he in the following year embarked for Gibraltar, in the 3rd battalion, under Major Innes, where he remained about six years. On his return to England, he obtained a furlough, and proceeded into the north to visit his relatives, when he found that, during his absence, his father had fallen into embarrassments, from

having neglected his business, and spent his property in his endeavors to obtain the prize offered by government to the discoverer of the longitude. Shortly after his return to Woolwich, his expertness in handling the Macaroni Gun elicited the applause of his sovereign, who, clapping him on the shoulder, exclaimed, "Fine young fellow—fine young fellow—make a man of you." From this time his abilities became daily more apparent, and his promotion was rapid. He first became bombardier in 1775, and was sent on a recruiting mission to Carlisle, where, being well known, and his success consequently great, he was continued for some months. He was engaged in various schemes connected with military pursuits during the succeeding seven years, and in 1782 we find him paymaster-sergeant and conductor of stores to the artillery encamped on South Sea Common. From South Sea Castle he was an eyewitness to the foundering of the *Royal George*, and from that time his mind became occupied in devising some plan for raising, or, should that be impracticable, for destroying the wreck. On the treaty of peace with America being ratified, he returned to Woolwich, where he was made inspector of the proof, which situation he held at the time of his death. Here he devoted his leisure hours to the scientific pursuits on which the whole energies of his mind were bent, and which his inventive genius enabled him to exhibit in a series of most extraordinary and valuable inventions. These it would be useless to attempt to describe in a mere biographical sketch, it must, therefore, suffice if some of the most remarkable be enumerated. And first, may be mentioned the "Sun Proof," by which the soundness of the interior of ordnance is scrutinised most effectually, and this proof is considered so decidedly superior to all others, that it is still believed to be used in the royal arsenal, and doubtless many a brave fellow is indebted for his lengthened existence to the security thus afforded him from the bursting of heavy artillery. A gyn, called "Bell's Gyn," (also still in use in the Royal Military Repository,) bears witness to his inventive genius, as do further, an effective petard, (a model of which may be seen at the laboratory, at Woolwich,) a method of destroying ordnance by means of a ponderous weight worked at a considerable altitude, and a variety of minor inventions and improvements. In 1791, a silver medal and a premium of five guineas were awarded to him by the Society of Arts, "For a safe crane, whereby the lives of persons descending or ascending precipices, wells, shafts of mines, &c., will be saved, although the line by which they are suspended may by accident be broken." In 1793, he received a further premium of twenty guineas from the same society, "For a gun

and harpoon on a new construction for taking whales, after satisfactory trials made therewith." (Vide Trans. of this Society.) Nor must be omitted the notice of his invention for destroying sunken bodies by the operation of gunpowder, the result of his reflections on the fate of the *Royal George*. Having proved its practicability by blowing up a sunken rock in the Frith of Forth, he further demonstrated its utility by experiments performed at Woolwich before the Duke of Richmond and other military gentlemen. Having directed a vessel to be built upon a scale of one inch to fifty of the thickness of the *Royal George's* side, he caused it to be sunk in the Thames, and with 50lbs. of gunpowder, afterwards conducted into her magazine, blew her to pieces. The experiment took place at high water, and answered every expectation of the inventor. A further experiment was made, which seemed to promise success, viz., the breaking of chains or booms laid across rivers, by means of a mine of gunpowder. (Vide Gents. Mag. for 1789, pages 753 and 947.) From this it is evident that, but for his premature death, the *Royal George* had long ago ceased to engage the inventive faculties of scientific minds. Colonel Pasley, who is now occupied on that wreck, to his highest honour be it spoken, has in a recent letter to the *Times*, generously awarded to Lieut. Bell his ready assent to the claim of his being the first projector of this scheme for her demolition, although he had not been previously acquainted with it.

"But the invention which of all others entitled him to a place as well among the "sons of genius," as among the benefactors of mankind, is that of the "Apparatus for rescuing shipwrecked mariners," for which the world generally considers itself indebted to Captain Manby. Without the least desire to advance any claim derogatory to the fair fame of the gallant captain, we must be permitted to adduce such proofs as are afforded (and these, we consider, are incontrovertible) that the real merit of the invention belongs solely, and in every material respect, to the departed genius whose memory we now seek to perpetuate. His untimely death afforded Captain Manby the opportunity of bringing it more decidedly before the public, and of obtaining the high emolument of which it was considered deserving; let him, therefore, permit the empty honour to gild the escutcheon of the undoubted proto-inventor. It is scarcely necessary to describe the apparatus, since there can be but few persons who are unacquainted with its nature and uses. The models of the whole apparatus, originally deposited by Lieut. Bell, may be inspected in the rooms of the Society for the Encouragement of Arts and Sciences. Several successful experiments were made at Woolwich before the Duke of Rich-

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mond, then Master-General of the Ordnance, and other distinguished and scientific individuals, when Lieut. Bell, by discharging a shell from a mortar, threw on shore a rope, by which he drew himself to the land with perfect ease, of which exploit a living witness now resides at Woolwich, namely, Mr. Laycock, who at that time served in the Royal Artillery. His success was so perfectly satisfactory, that, in 1792, a premium of fifty guineas was awarded to him for the invention by the Society of Arts. It would seem that Captain Manby's claim to the originating of this contrivance rested solely upon the subtle distinction of his throwing a line across the ship from a mortar on shore, whilst Lieut. Bell's has been thought to suggest the throwing it from the vessel *only*. The following extract from Lieut. Bell's observations, transmitted to the Society of Arts, and published in their transactions, proves that his plan comprehended both these methods:—"there is every reason to conclude that this contrivance would be very useful at all ports of difficult access, both at home and abroad, where ships are liable to strike ground before they enter the harbour, as Shields Bar, and other similar situations, when a line might be *thrown over the ship*, which might probably be the means of saving both lives and property; and, moreover, if a ship were driven on shore near such a place, the apparatus might easily be removed to afford assistance; and the whole performance is so exceedingly simple, that any person once seeing it done, would not want any further instruction. (Vide Trans. of the Society of Arts, vol. 25, page 135.)

"In addition to the above convincing proofs of the right of Lieut. Bell to the honour of this invention, two extracts are subjoined—First, from Lieut. General Farrington's letter of 19th January, 1808, transmitting report of the committee of field officers of artillery at Woolwich on the Manby apparatus, in which the lieutenant-general states, "that this invention was brought forward by the late Lieut. Bell, of the Royal Artillery, nearly fourteen years since; his idea was to project the rope from the ship to the shore, which is assuredly the method most to be depended upon, as the vessel in that case carries the means with it, and need not rely on any fortuitous assistance from the shore." And, secondly, an extract from the letter of Colonel Ramsay, dated October, 1808, containing a report of field officers of artillery on the Manby apparatus, in which the colonel states, "that Lieut. Bell was presented with a premium from the Society of Arts for a similar application of ordnance to come from the ship to the shore, and for having also suggested its utility in *projecting the rope from the land to vessels* in danger of being wrecked." (Vide report

of the committee of the House of Commons on the application of Captain Manby for remuneration for the apparatus.)

“If any proof be required in addition to the above of the true person entitled to the merit of this valuable invention, it might be found in the fact of the House of Commons having, in the year 1815, voted the sum of £500 to Mrs. E. Whitfield, as a reward for the ingenuity of her father, in discovering the apparatus for rescuing persons in danger of losing their lives by shipwrecks.

“Little more remains to be said; his reputation as a man of sound practical science was now fully established, and in 1793 he was presented by the Duke of Richmond, who had frequently witnessed his experiments and unreservedly expressed his high opinion of his abilities, with a commission as second lieutenant in the Royal Artillery. Somewhere about this time he was despatched on a secret expedition, which had for its object the destruction of the Dutch fleet in the Texel, but which was abandoned. In January, 1794, he was promoted to the rank of first lieutenant in the Invalid Battalion in the same regiment, and in this station continued till his death. Lient. Bell died suddenly at Queenborough, on the 1st June, 1798, in the height of his successful career, whilst engaged in improving and fitting out the fire-ships there, after having devoted his best energies to the service of his country, to the interests of science, and to the cause of humanity, without having reaped those pecuniary benefits which he might have reasonably anticipated as the reward of untiring exertions, terminating in a series of valuable discoveries.”—*United Service Journal*.

Photogenic Art—Engraving.

It was stated last week, that Dr. Berres, of Vienna, had discovered a method of fixing the impressions produced by Daguerreotypy, by means of which these productions can be employed instead of engraved plates, and copies therefrom printed, as in the course of the ordinary copper plates, &c. We now give a copy of the doctor's account of his discovery:—

“It was announced in the Vienna Gazette of the 18th of April last, that I had succeeded in discovering a method by which I was enabled both permanently to fix the pictures produced by the method of Daguerre, and to render them available to all the purposes of etchings upon copper, steel, &c. from which copies might be struck off to any extent, as in the case of ordinary engraved productions; and it was stated in the same newspaper, that I proposed bringing my discovery immediately before the public.

“As a member of this distinguished society, I consider it my duty, first, to make known to this learned body a discovery which creates so much hope, and which promises so great a benefit to the arts and sciences. The well known expenses and difficulties attendant on the publication of an extensive work, requiring engravings as illustrations, led me, in the first instance, to hope that I might be enabled to render the discovery of Daguerre available by improvements, to represent and fix the objects necessary to my work; and the first view of a heliographed picture aroused in me the desire also to represent, in the same manner, microscopic objects, although attempts, with the strongest lamplights, to produce engravings or etchings had been unsuccessful, and the idea abandoned as hopeless, until revived by a sight of the hydro-oxygen gas microscope of M. Schuh, of Berlin, an instrument which, in its power and clearness, has never before been equalled or even approached. On the 27th February last, I had the honour of laying before the learned body the results of the united investigations of my distinguished colleague, Professor de Ettingshausen, and myself upon this subject, and the perfectly successful experiments of pictures prepared through the process of photography upon microscopic objects. Many specimens of the results of our researches and successful attempts to employ photography for scientific and useful purposes are now placed before you for examination. Through this new method, the Daguerreotype is rendered more extensively available for scientific uses. Every object which is discernible to the eye with clearness, can, in the future, through the means of the iodined silver plates, be minutely etched, and, true to nature, (for she is herself the artist!) be copied with the minutest exactness. But the beautiful representations which we are able to produce through the means of the Daguerreotype are liable to so many injuries, and are so delicate, fragile, and evanescent, that they never can be rendered available for illustrating works of science and other useful purposes.

“In a Petersburg newspaper, of March last, I first saw an account of some attempts to bring the Daguerreotype process into general use. In the meantime, M. Daguerre had declared, before the institute of Paris, the complete failure of all his attempts, by means of etching, to obtain the impression even of a single copy.

“The experiments at St. Petersburg, and the hope of eventual success, urged me to attempt to make some use of the Daguerreotype pictures; and I began, at the commencement of this month, my series of experiments. Without recapitu-

lating all these, in which I was assisted with cordial zeal by M. Francis Kratochwila (a gentleman in the employ of government), and by M. Schuh, who placed at my disposal an immense number of Daguerreotype plates, and before I come to an explanation of the process by which I render these Daguerreotype pictures permanent and capable of further use, I consider it necessary to lay before this learned body the following observations:—

“1st, With the copper plates, as used at present in the Daguerreotype process, we can effect only the permanently fixing, never the etching and printing of copies therefrom.

“2nd, For the heliographic etchings, it is necessary that the picture be produced with the required intensity, upon pure chemical silver plates.

“3rd, The etching of the Daguerreotype picture is produced through the influence of nitric acid, to be explained hereafter.

“4th, For the permanently fixing of the Daguerreotype impression, a galvanic power is necessary.

“5th, For the changing of the Daguerreotype picture into a deep metal etching, so as to be used as a means of printing, the chemical process of etching is of itself sufficient.

“My newly discovered method of managing the Daguerreotype pictures may be divided into two processes:

“1st, The permanently fixing the design.

“2nd, The changing of the design, when once permanently fixed, into an etching upon the plate.

“The method of permanently fixing the Daguerreotype picture with a transparent metal coating consists in the following process:—

“I take the pictures produced in the usual manner, by the Daguerreotype process, hold them for some minutes over a moderately-warmed nitric acid vapour, or steam, and then lay them in nitric acid of 13° to 14° Reaumur, in which a considerable quantity of copper or silver, or both together, has been previously dissolved. Shortly after being placed therein, a precipitate of metal is formed, and can now be changed to what degree of intensity I desire. I then take the heliographic picture coated with metal, place it in water, clean it, dry it, polish it with chalk or magnesia and a dry cloth or soft leather. After this process the coating will become clean, clear, and transparent, so that the picture can again be easily seen. The greatest care and attention are required in preparing the Daguerreotype impressions intended to be printed from. The picture must be carefully freed from iodine, and prepared upon a plate of the most chemically pure silver.

“That the production of this picture should be certain of

succeeding, according to the experiments of M. Kratochwila, it is necessary to unite a silver with a copper plate; while, upon other occasions, without being able to explain the reasons, deep etchings or impressions are produced, without the assistance of the copper plate, upon pure silver plate.

“The plate will now, upon the spot where acid ought not to have dropped, be varnished; then held for one or two minutes over a weak warm vapour or steam, of 25° to 30° (Reaumur), of nitric acid, and then a solution of gum arabic, of the consistence of honey, must be poured over it, and it must be placed in an horizontal position, with the impression uppermost, for some minutes. Then place the plate, by means of a kind of double pincette (whose ends are protected by a coating of asphalt or hard wood), in nitric acid, at 12° or 13° (Reaumur). Let the coating of gum slowly melt off or disappear, and commence now to add, though carefully and gradually, at a distance from the picture, a solution of nitric acid, of from 25° to 33° , for the purpose of deepening or increasing the etching power of the solution. After the acid has arrived at 16° to 17° (Reaumur), and gives off a peculiar biting vapour, which powerfully affects the sense of smelling, the metal becomes softened, and then, generally, the process commences changing the shadow upon the plate into a deep engraving or etching. This is the decisive moment, and upon it must be bestowed the greatest attention. The best method of proving if the acid be strong enough is to apply a drop of the acid in which the plate now lies to another plate; if the acid make no impression, it is, of course, necessary to continue adding nitric acid; if, however, it corrode too deeply, then it is necessary to add water, the acid being too strong. The greatest attention must be bestowed upon this process. If the acid has been too potent, a fermentation or white froth will cover the whole picture, and thus not alone the surface of the picture, but also the whole surface of the plate, will quickly be corroded. When, by a proper strength of the etching powers of the acid, a soft and expressive outline of the picture shall be produced, then may we hope to finish this undertaking favorably. We have now only to guard against an ill-measured division of the acid, and the avoidance of a precipitate. To attain this end, I frequently lift the plate out of the fluid, taking care that the etching power shall be directed to whatever part of the plate it may have worked the least, and seek to avoid the bubbles and precipitate by a gentle movement of the acid.

“In this manner the process can be continued to the proper points of strength and clearness of etching required upon the plates from which it is proposed to print. I believe that a man

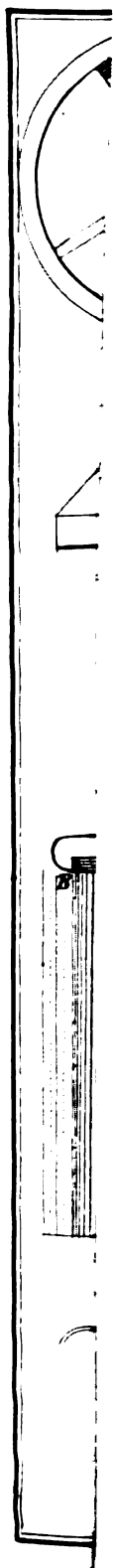
of talent, who might be interested with this art of etching, and who had acquired a certain degree of dexterity in preparing for it, would very soon arrive at the greatest clearness and perfection; and, from my experience, I consider that he would soon be able to simplify the whole process. I have tried very often to omit the steaming and the gum arabic; but the result was not satisfactory, or the picture very soon after was entirely destroyed, so that I was compelled again to have recourse to them.

“The task which I have undertaken is now fully performed, by placing in the hands of this learned body my method of etching and printing from the Daguerreotype plates, which information, being united to the knowledge and mathematical experience we already possess, and published to the world, may open a road to extensive improvement in the arts and sciences. By thus laying open my statement to the scientific world, I hope to prove my devotion to the arts and sciences, which can end only with my life.”

Discovery of the Mariners' Compass.

“Much interest must for ever attach to the discovery of this instrument, and yet there are few subjects concerning which is less known. For a period, the honour of the invention was ascribed to Gioia, a pilot, or ship captain, born at Pasitano, a small village situated near Malphi, or Amalfi, about the end of the thirteenth century. His claims, however, have been disputed. According to some, he did not invent but improve it; and according to others, he did neither. Much learning and labor have been bestowed upon the subject of the discovery. It has been maintained by one class, that even the Phœnicians were the inventors; by another, that the Greeks and Romans had a knowledge of it. Such notions, however, have been completely refuted. One passage nevertheless, of a very remarkable character, occurs in the work of Cardinal de Vitry, bishop of Ptolemais, in Syria. He went to Palestine during the fourth crusade, about the year 1204; he returned afterwards to Europe, and subsequently went back to the Holy Land, where he wrote his work entitled “*Historia Orientalis*,” as nearly as can be determined, between the years 1215 and 1220. In chap. xci of that work he has this singular passage:—“the iron needle, after contact with the loadstone, constantly turns to the north star, which, as the axis of the firmament, remains immoveable, whilst the others revolve; and hence it is essentially necessary to those navi-

gating on the ocean." These words are as explicit as they are extraordinary; they state a fact, and announce a use. The thing, therefore, which essentially constitutes the compass, must have been known long before the birth of Gioia. In addition to this fact, there is another equally fatal to his claims as the original discoverer: it is now settled beyond a doubt, that the Chinese were acquainted with the compass long before the Europeans. It is certain that there are allusions to the magnetic needle in the traditionary period of Chinese history, about 2,600 years before Christ; and a still more credible account of it is found in the reign of Ching-wang, of the Chow dynasty, before Christ 1,114. All this, however, may be granted, without in the least impairing the just claims of Gioia to the gratitude of mankind. The truth appears to be this: the position of Gioia, in relation to the compass, was precisely that of Watt in relation to the steam engine—the element existed, he augmented its utility. The compass used by the mariners in the Mediterranean, during the twelfth and thirteenth centuries, was a very uncertain and unsatisfactory apparatus. It consisted only of a magnetic needle floating in a vase or basin by means of two straws or a bit of cork supporting it on the surface of the water. The compass used by the Arabians in the thirteenth century was an instrument of exactly the same description. Now the inconvenience and inefficiency of such an apparatus are obvious; the agitation of the ocean, and the tossing of the vessel, might render it useless in a moment. But Gioia placed the magnetized needle on a pivot, which permits it to turn to all sides with facility. Afterwards, it was attached to a card, divided into thirty-two points, called *Rose des Vents*; and then the box containing it was suspended in such a manner, that however the vessel might be tossed, it would always remain horizontal. The result of an investigation participated by men of various nations, and possessing the highest degree of competency, may thus be stated. The discovery of the directive virtue of the magnet was made anterior to the time of Gioia. Before that period, navigators, both in the Mediterranean and Indian seas, employed the magnetic needle; but Gioia, by his invaluable improvement in the principle of suspension, is fully entitled to the honor of being considered the real inventor, in Europe, of the compass as it now exists."—*Campbell's Maritime Discovery.*



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THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

AUGUST, 1840.

XII.—*Experimental Researches in Electricity.—Twelfth Series.* By MICHAEL FARADAY, Esq. D. C. L. F. R. S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acad. of Sciences, Paris, Petersburg, Florence, Copenhagen, Berlin, &c. &c.

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§ 18. *On Induction (continued).* ¶ vii. *Conduction, or conductive discharge.* ¶ viii. *Electrolytic discharge.* ¶ ix. *Disruptive discharge—Insulation—Spark—Brush—Difference of discharge at the positive and negative surfaces of conductors.*

1318. I proceed now, according to my promise, to examine, by the great facts of electrical science, that theory of induction which I have ventured to put forth (1165. 1295. &c.) The principle of induction is so universal that it pervades all electrical phenomena; but the general case which I purpose at present to go into, consists of insulation traced into and terminating with discharge, with the accompanying effects. This case includes the various *modes* of discharge, and also the condition and characters of a current; the elements of magnetic action being amongst the latter. I shall necessarily have occasion to speak theoretically, and even hypothetically; and though these papers profess to be experimental researches, I hope that, considering the facts and investigations contained in the last series in support of the particular view advanced,
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I shall not be considered as taking too much liberty on the present occasion, or as departing too far from the character which they ought to have, especially as I shall use every opportunity which presents itself of returning to that strong test of truth, experiment.

1319. Induction has as yet been considered in these papers only in cases of insulation;—opposed to insulation is *discharge*. The action or effect which may be expressed by the general term *discharge*, may take place, as far as we are aware at present, in several modes: Thus, that which is simply called *conduction* involves no chemical action, and apparently no displacement of the particles concerned. A second mode may be called *electrolytic discharge*; in it chemical action does occur, and particles must, to a certain degree, be displaced. A third mode, namely, that by sparks or brushes may, because of its violent displacement of the particles of the *dielectric* in its course, be called the *disruptive discharge*; and a fourth may, perhaps, be conveniently distinguished for a time by the words *convection*, or *carrying discharge*, being that in which discharge is effected either by the carrying power of solid particles, or those of gases and liquids. Hereafter, perhaps, all these modes may appear as the result of one common principle, but at present they require to be considered apart; and I will now speak of the *first* mode, for amongst all the forms of discharge that which we express by the term conduction appears the most simple and the most directly in contrast with insulation.

¶ vii. *Conduction, or conductive discharge.*

1320. Though assumed to be essentially different, yet neither Cavendish nor Poisson attempt to explain by, or even state in, their theories, what the essential difference between insulation and conduction is. Nor have I anything, perhaps, to offer in this respect, *except* that, according to my view of induction, both it and conduction depend upon the same molecular action of the dielectrics concerned; are only extreme degrees of *one common condition* or effect; and in any sufficient mathematical theory of electricity must be taken as cases of the same kind. Hence the importance of the endeavour to shew the connexion between them under my theory of the electrical relations of contiguous particles.

1321. Though the action of the insulating dielectric in the charged Leyden jar, and that of the wire in discharging it, may seem very different, they may be associated by numerous intermediate links, which carry us on from one to the other, leaving, I think, no necessary connexion unsupplied. We may ob-

serve some of these in succession for information respecting the whole case.

1322. Spermaceti has been examined and found to be a dielectric, through which induction can take place (1240. 1246), its specific inductive capacity being about or above 1.8 (1279,) and the inductive action has been considered in it, as in all other substances, an action of contiguous particles.

1323. But spermaceti is also a *conductor*, though in so low a degree that we can trace the process of conduction, as it were, step by step through the mass (1247.); and even when the electric force has travelled through it to a certain distance, we can, by removing the coercitive (which is at the same time the inductive) force, cause it to return upon its path and reappear in its first place (1245. 1246.) Here induction appears to be a necessary preliminary to conduction. It, of itself, brings the contiguous particles of the dielectric into a certain condition, which, if retained by them, constitutes *insulation*, but if lowered by the communication of power from one particle to another, constitutes *conduction*.

1324. If *glass* or *shell-lac* be the substances under consideration, the same capabilities of suffering either induction or conduction through them appear (1233. 1239. 1247.), but not in the same degree. The conduction almost disappears (1239. 1242.); the induction therefore is sustained, i. e. the polarized state into which the inductive force has brought the contiguous particles is retained, there being little discharge action between them, and therefore the *insulation* continues. But, what discharge there is, appears to be consequent upon that condition of the particles into which the induction throws them; and thus it is that ordinary insulation and conduction are closely associated together, or rather are extreme cases of one common condition.

1325. In ice or water we have a better conductor than spermaceti, and the phenomena of induction and insulation therefore quickly disappear, because conduction quickly follows upon the assumption of the inductive state. But let a plate of cold ice have metallic coatings on its sides, and connect one of these with a good electrical machine in work, and the other with the ground, and it then becomes easy to observe the phenomena of induction through the ice, by the electrical tension which can be obtained and continued on both the coatings (419. 426.) For although that portion of power which at one moment gave the inductive condition to the particles is at the next lowered by the consequent discharge due to the conductive act, it is succeeded by another portion of force from the machine to restore the inductive state. If the ice be converted

into water, the same succession of actions can be just as easily proved, provided the water be distilled, and, (if the machine be not powerful enough) a voltaic battery be employed.

1326. All these considerations impress my mind strongly with the conviction, that insulation and ordinary conduction cannot be properly separated when we are examining into their nature; that is, into the general law or laws under which their phenomena are produced. They appear to me to consist in an action of contiguous particles, dependent on the forces developed in electrical excitement; these forces bring the particles into a state of tension or polarity, which constitutes both *induction* and *insulation*; and being in this state, the continuous particles have a power or capability of communicating their forces one to the other, by which they are lowered, and discharge occurs. Every body appears to discharge (444); but the possession of this capability in a *greater or smaller degree* in different bodies, makes them better or worse conductors, worse or better insulators; and both *induction* and *conduction* appear to be the same in their principle and action (1320.), except that in the latter an effect common to both is raised to the highest degree, whereas in the former it occurs in the best cases, in only an almost insensible quantity.

1327. That in our attempts to penetrate into the nature of electrical action, and to deduce laws more general than those we are at present acquainted with, we should endeavor to bring apparently opposite effects to stand side by side in harmonious arrangement, is an opinion of long standing, and sanctioned by the ablest philosophers. I hope, therefore, I may be excused the attempt to look at the highest cases of conduction as analogous to, or even the same in kind with, those of induction and insulation.

1328. If we consider the slight penetration of sulphur (1241. 1242.) or shell-lac (1234.) by electricity, or the feebler insulation sustained by spermaceti (1279. 1240.), as essential consequences and indications of their *conducting* power, then may we look on the resistance of metallic wires to the passage of electricity through them as *insulating* power. Of the numerous well known cases fitted to shew this resistance in what are called the perfect conductors, the experiments of Professor Wheatstone best serve my present purpose, since they were carried to such an extent as to shew that *time* entered as an element into the conditions of conduction* even in metals. When discharge was made through a copper wire 2640 feet in length, and 1-15th of an inch in diameter, so that the lu-

* Philosophical Transactions, 1834, p. 583.

minous sparks at each end of the wire, and at the middle, could be observed in the same place, the latter was found to be sensibly behind the two former in time, they being by the conditions of the experiment, simultaneous. Hence a proof of retardation; and what reason can be given why this retardation should not be of the same kind as that in spermaceti, or in lac, or sulphur? But as, in them, retardation is insulation, and insulation is induction, why should we refuse the same relation to the same exhibitions of force in the metals.

1329. We learn from the experiment, that if *time* be allowed retardation is gradually overcome; and the same thing obtains for the spermaceti, the lac, and glass; give but time in proportion to the retardation, and the latter is at last vanquished. But if that be the case, and all the results are alike in kind, the only difference being in the length of time, why should we refuse to metals the previous inductive action, which is admitted to occur in the other bodies? The diminution of *time* is no negation of the action; nor is the lower degree of tension requisite to cause the forces to traverse the metal, as compared to that necessary in the cases of water, spermaceti, or lac. These differences would only point to the conclusion, that in metals the particles under induction can transfer their forces when at a lower degree of tension or polarity, and with greater facility than in the instances of the other bodies.

1330. Let us look at Mr. Wheatstone's beautiful experiment in another point of view. If, leaving the arrangement at the middle and two ends of the long copper wire unaltered, we remove the two intervening portions and replace them by wires of iron or platina, we shall have a much greater retardation of the middle spark than before. If, removing the iron, we were to substitute for it only five or six feet of water in a cylinder of the same diameter as the metal, we should have still greater retardation. If from water we passed to spermaceti, either directly or by gradual steps through other bodies, (even though we might vastly enlarge the bulk, for the purpose of evading the occurrence of a spark elsewhere (1331.) than at the three proper intervals,) we should have still greater retardation, until at last we might arrive, by degrees so small as to be inseparable from each other, at actual and permanent insulation. What, then, is to separate the principle of these two extremes, perfect conduction and perfect insulation, from each other; since the moment we leave in the smallest degree perfection at either extremity, we involve the element of perfection at the opposite end? Especially too, as we have not in nature the case of perfection either at one extremity or the other, either of insulation or conduction.

1331. Again, to return to this beautiful experiment in the various forms which may be given to it: the forces are not all in the wire (after they have left the Leyden jar) during the whole time (1328.) occupied by the discharge; they are disposed in part through the surrounding dielectric under the well-known form of induction; and if that dielectric be air, induction takes place from the wire through the air to surrounding conductors, until the ends of the wire are electrically related through its length and discharge has occurred, i. e. for the *time* during which the middle spark is retarded beyond the others. This is well shewn by the old experiment, in which a long wire is so bent that two parts (Plate 2, fig. 1. *a. b.*) near its extremities shall approach within a short distance, as a quarter of an inch, of each other in the air. If the discharge of a Leyden jar, charged to a sufficient degree, be sent through such a wire, by far the largest portion of the electricity will pass as a spark across the air at the interval, and not by the metal. Does not the middle part of the wire, therefore, act here as an insulating medium, though it be of metal? and is not the spark through the air an indication of the tension (simultaneous with *induction*) of the electricity in the ends of this single wire? Why should not the wire and the air both be regarded as dielectrics; and the action at its commencement, and whilst there is tension, as an inductive action? If it acts through the contorted lines of the wire, so it also does in curved lines through air (1219. 1224.), and other insulating dielectrics (1228.); and we can apparently go so far in the analogy, whilst limiting the case to the inductive action only, as to shew that amongst insulating dielectrics some lead away the lines of force from others (1229.), as the wire will do from worse conductors, though in it the principal effect is no doubt due to the ready discharge between the particles whilst in a low state of tension. The retardation is for the time insulation; and it seems to me we may just as fairly compare the air at the interval *a, b*, (fig. 1.) and the wire in the circuit, as two bodies of the same kind and acting upon the same principles, as far as the first inductive phenomena are concerned, notwithstanding the different forms of discharge which ultimately follow,* as we may compare, according to Coulomb's investigations,† *different lengths* of different insulating bodies required to produce the same amount of insulating effect.

* These will be examined hereafter (1348, &c.)

† *Memoires de l' Academie*, 1785, p. 612, or *Ency. Britann.* First Supp. vol. 1, p. 611.

1332 This comparison is still more striking when we take into consideration the experiment of Mr. Harris, in which he stretched a fine wire across a glass globe, the air within being rarefied.* On sending a charge through the joint arrangement of metal and rare air, as much, if not more, electricity passed by the latter as by the former. In the air, rarefied as it was, there can be no doubt the discharge was preceded by induction (1284.); and to my mind all the circumstances indicate that the same was the case with the metal; that, in fact, both substances are dielectrics, exhibiting the same effects in consequence of the action of the same causes, the only variation being one of degree in the different substances employed.

1333. Judging on these principles, velocity of discharge through the *same wire* may be varied greatly by attending to the circumstances which cause variations of discharge through spermaceti or sulphur. Thus, for instance, it must vary with the tension or intensity of the first urging force (1234. 1240.), which tension is charge and induction. So if the two ends of the wire, in Professor Wheatstone's experiment, were immediately connected with two large insulated metallic surfaces exposed to the air, so that the primary act of induction, after making the contact for discharge, might be in part removed from the internal portion of the wire at the first instant, and disposed for the moment on its surface jointly with the air and surrounding conductors, then I venture to anticipate that the middle spark would be more retarded than before; and if these two plates were the inner and outer coating of a large jar or a Leyden battery, then the retardation of that spark would be still greater.

1334. Cavendish was perhaps the first to shew distinctly that discharge was not always by one channel,† but, if several are present, by many at once. We may make these different channels of different bodies, and by proportioning their thicknesses and lengths, may include such substances as air, lac, spermaceti, water, protoxide of iron, iron and silver, and by *one* discharge make each convey its proportion of the electric force. Perhaps the air ought to be excepted, as its discharge by conduction is questionable at present; but the others may all be limited in their mode of discharge to pure conduction. Yet several of them suffer previous induction, precisely like the induction through the air, it being a necessary preliminary to their discharging action. How can we therefore separate

* Philosophical Transactions, 1834, p. 242.

† Philosophical Transactions, 1776, p. 197.

any one of these bodies from the others, as to the *principles and mode* of insulating and conducting, except by mere degree? All seem to me to be dielectrics acting alike, and under the same common laws.

1335. I might draw another argument in favor of the general sameness, in nature and action, of good and bad conductors (and all the bodies I refer to are conductors more or less), from the perfect equipoise in action of very different bodies when opposed to each other in magneto-electric inductive action, as formerly described (213.), but am anxious to be as brief as is consistent with the clear examination of the probable truth of my views.

1336. With regard to the possession by the gases of any conducting power of the simple kind now under consideration, the question is a very difficult one to determine at present. Experiments seem to indicate that they do insulate certain low degrees of tension perfectly, and that the effects which may have appeared to be occasioned by *conduction* have been the result of the carrying power of the charged particles, either of the air or of dust, in it. It is equally certain, however, that with higher degrees of tension or charge they discharge to one another, and that is conduction. If they possess the power of insulating a certain low degree of tension continuously and perfectly, such a result may be due to their peculiar physical state, and the condition of separation under which their particles are placed. But in that, or in any case, we must not forget the fine experiments of Cagniard de la Tour,* in which he has shewn that liquids and their vapors can be made to pass gradually into each other, to the entire removal of any marked distinction of the two states. Thus, hot dry steam and cold water pass by insensible gradations into each other; yet the one is amongst the gases as an insulator, and the other a comparatively good conductor. As to conducting power, therefore, the transition from metals even up to gases is gradual; substances make but one series in this respect, and the various cases must come under one condition and law (444.) The specific differences of bodies as to conducting power only serve to strengthen the general argument that conduction, like insulation, is a result of induction, and is an action of contiguous particles.

1337. I might go on now to consider induction and its concomitant, *conduction*, through mixed dielectrics, as, for instance, when a charged body, instead of acting across air to a distant uninsulated conductor, acts jointly through it and

* Annales de Chimie, xxi. pp.127.178. or Quarterly Journal of Science, xv.145.

an interposed insulated conductor. In such a case, the air and the conducting body are the mixed dielectrics; and the latter assumes a polarized condition as a mass, like that which my theory assumes *each particle* of the air to possess at the same time. But I fear to be tedious in the present condition of the subject, and hasten to the consideration of other matter.

1338. To sum up, in some degree, what has been said, I look upon the first effect of an excited body upon neighbouring matters to be the production of a polarized state of their particles, which constitutes *induction*; and this arises from its action upon the particles in immediate contact with it, which again act upon those contiguous to them, and thus the forces are transferred to a distance. If the induction remain undiminished, then perfect insulation is the consequence; and the higher the polarized condition which the particles can acquire or maintain, the higher is the intensity which may be given to the acting forces. If, on the contrary, the contiguous particles, upon acquiring the polarized state, have the power to communicate their forces, then conduction occurs, and the tension is lowered, conduction being a distinct act of discharge between neighbouring particles. The lower the state of tension at which this discharge between the particles of a body takes place, the better conductor is that body. In this view, insulators may be said to be bodies whose particles can retain the polarized state: whilst conductors are those whose particles cannot be permanently polarized. If I be right in my view of induction, then I consider the reduction of these two effects (which have been so long held distinct) to an action of contiguous particles obedient to one common law, as a very important result; and, on the other hand, the identity of character which the two acquire when viewed by the theory (1326.), is additional presumptive proof in favor of the correctness of the latter.

1339. That heat has great influence over simple conduction is well known (445.), its effect being, in some cases, almost an entire change of the characters of the body (432. 1340.) Harris has, however, shewn that it in no respect affects gaseous bodies, or at least air;* and Davy has taught us that, as a class, metals have their conducting power *diminished* by it.†

1340. I formerly described a substance, sulphuret of silver, whose conducting power was increased by heat (433. 437. 438.); and I have since then met with another as strongly

* Philosophical Transactions, 1834, p. 230.

† Ibid. 1821, p. 431.

affected in the same way: this is fluoride of lead. When a piece of that substance, which had been fused and cooled, was introduced into the circuit of a voltaic battery, it stopped the current. Being heated, it acquired conducting powers before it was visibly red hot in daylight; and even sparks could be taken against it whilst still solid. The current alone then raised its temperature (as in the case of sulphuret of silver) until it fused, after which it seemed to conduct as well as the metallic vessel containing it; for whether the wire used to complete the circuit touched the fused fluoride only, or was in contact with the platina on which it was supported, no sensible difference in the current was observed. During all the time there was scarcely a trace of decomposing action on the fluoride, and what did occur seemed referable to the air and moisture of the atmosphere, and not to electrolytic action.

1341. I have now very little doubt that periodide of mercury (414. 448. 691.) is a case of the same kind, and also corrosive sublimate (692). I am also inclined to think, since making the above experiments, that the anomalous action of the protoxide of antimony, formerly observed and described (693. 801.), may be referred in part to the same cause.

1342. I have no intention at present of going into the particular relation of heat and electricity, but we may hope hereafter to discover by experiment the law which probably holds together all the above effects with those of the *evolution* and the *disappearance* of heat by the current, and the striking and beautiful results of thermo-electricity, in one common bond.

¶ viii. *Electrolytic Discharge.*

1343. I have already expressed in a former paper (1164.) the view by which I hope to associate ordinary induction and electrolyzation. Under that view, the discharge of electric forces by electrolyzation is rather an effect superadded, in a certain class of bodies, to those already described as constituting induction and insulation, than one independent of, and distinct from, these phenomena.

1344. Electrolytes, as respects their insulating and conducting forces, belong to the general category of bodies (1320. 1334.); and if they are in the solid state (as nearly all can assume that state), they retain their place, presenting then no new phenomenon (426, &c.); or if one occur being in so small a proportion as to be almost unimportant. When liquefied, they also belong to the same list whilst the electric intensity is below a certain degree; but at a given intensity (910. 912. 1007.), fixed for each, and very low in all known cases,

they play a new part, causing discharge in proportion (783.) to the developement of certain chemical effects of combination and decomposition; and at this point, move out from the the general class of insulators and conductors, to form a distinct one by themselves. The former phenomena have been considered (1320. 1338.); it is the latter which have now to be revised, and used as a test of the proposed theory of induction.

1345. The theory assumes, that the particles of the dielectric (now an electrolyte) are in the first instance brought, by ordinary inductive action, into a polarized state, and raised to a certain degree of tension or intensity before discharge commences; the inductive state being, in fact, a *necessary preliminary* to discharge. By taking advantage of those circumstances which bear upon the point, it is not difficult to increase the tension indicative of this state of induction, and so make the state itself more evident. Thus, if distilled water be employed, and a long narrow portion of it placed between the electrodes of a powerful voltaic battery, we have at once indications of the intensity which can be sustained at these electrodes by the inductive action through the water as a dielectric, for sparks may be obtained, gold leaves diverged, and Leyden bottles charged at their wires. The water is in the condition of the spermaceti (1322. 1323.), a bad conductor and a bad insulator; but what it does insulate is by virtue of inductive action, and that induction is the preparation for, and precursor of, discharge (1338.)

1346. The induction and tension which appear at the limits of the portion of water in the direction of the current, are only the sums of the induction and tension of the contiguous particles between those limits; and the limitation of the inductive tension, to a certain degree shews (time entering in each case as an important element of the result), that when the particles have acquired a certain relative state, *discharge*, or a transfer of forces equivalent to ordinary conduction, takes place.

1347. In the inductive condition assumed by water before discharge comes on, the particles polarized are the particles of the *water*, that being the dielectric used; but the discharge between particle and particle is not, as before, a mere interchange of their powers or forces at the polar parts, but an actual separation of them into their two elementary particles, the oxygen travelling in one direction, and carrying with it its amount of the force it had acquired during the polarization, and the hydrogen doing the same thing in the other direction, until they each meet the next approaching particle, which is

in the same electrical state with that they have left, and by association of their forces with it, produce what constitutes discharge. This part of the action may be regarded as a carrying one (1319.), performed by the constituent particles of the dielectric. The latter is always a compound body (664. 823.); and by those who have considered the subject and are acquainted with the philosophical view of transfer which was first put forth by Grotthuss,* its particles may easily be compared to a series of metallic conductors under inductive action, which, whilst in that state, are divisible into these elementary movable halves.

1348. Electrolytic discharge depends, of necessity, upon the non-conduction of the dielectric as a whole, and there are two steps or acts in the process: first a polarization of the molecules of the substance, and then a lowering of the forces by the separation, advance in opposite directions, and recombination of the elements of the molecules, they being, as it were, the halves of the originally polarized conductors or particles.

1349. These views of the decomposition of electrolytes and the consequent effect of discharge, which, as to the particular case, are the same with those of Grotthuss (481.) and Davy (482.), though they differ from those of Biot (487.) De la Rive (490.), and others, seem to me to be fully in accordance not merely with the theory I have given of induction generally (1165.), but with all the known *facts* of common induction, conduction, and electrolytic discharge; and in that respect help to confirm, in my mind, the truth of the theory set forth. The new mode of discharge which electrolyzation presents must surely be an evidence of the *action of contiguous particles*; and as this appears to depend directly upon a previous inductive state, which is the same with common induction, it greatly strengthens the argument which refers induction in all cases to an action of contiguous particles also (1295, &c.).

1350. As an illustration of the condition of the polarized particles in a dielectric under induction, I may describe an experiment. Put into a glass vessel some clear rectified oil of turpentine, and introduce two wires passing through glass tubes where they are at the surface of the fluid, and terminating either in balls or points. Cut some very clean dry white silk into small particles, and put these also into the liquid; then electrify one of the wires by an ordinary machine and discharge by the other. The silk will immediately gather from all parts of the liquid, and form a band of particles

* *Annals de Chimie*, lxxiii. 60 and lxxiii. 20.

reaching from wire to wire, and if touched by a glass rod will shew considerable tenacity; yet the moment the supply of electricity ceases, the band will fall away and disappear by the dispersion of its parts. The *conduction* by the silk is in this case very small; and after the best examination I could give to the effects, the impression on my mind is, that the adhesion of the whole is due to the polarity which each filament acquires, exactly as the particles of iron between the poles of a horse-shoe magnet are held together in one mass by a similar disposition of forces. The particles of silk therefore represent to me the condition of the molecules of the dielectric itself, which I assume to be polar, just as that of the silk is. In all cases of conductive discharge the contiguous polarized particles of the body are able to effect a neutralization of their forces with greater or less facility, as the silk does also in a very slight degree. Further we are not able to carry the parallel, except in imagination; but if we could divide each particle of silk into two halves, and let each half travel until it met and united with the next half in an opposite state, it would then exert its carrying power (1347.), and so far represent electrolytic discharge.

1351. Admitting that electrolytic discharge is a consequence of previous induction, then how evidently do its numerous cases point to induction in curved lines (1216.), and to the divergence or lateral action of the lines of inductive force (1231.), and so strengthen that part of the general argument in the former paper! If two balls of platina, forming the electrodes of a voltaic battery, are put into a large vessel of dilute sulphuric acid, the whole of the surfaces are covered with the respective gases in beautifully regulated proportions, and the mind has no difficulty in conceiving the direction of the curved lines of discharge, and even the intensity of force of the different lines by the quantity of gas evolved upon the different parts of the surface. Hence the general effects of diffusion; the appearance of the anions or cations round the edges on the further side of the electrodes when in the form of plates; the manner in which the current or discharge will follow all the forms of electrolyte, however contorted. Hence the effects which Nobili has so well examined and described* in his papers on the distribution of currents in conducting masses. All these effects indicate the direction of the currents and discharges which occur in and through the dielectrics, and these are in every case *preceded* by equivalent inductive actions of the contiguous particles.

* *Bibliothèque Universelle*, 1835, lix. 263, 416.

1352. Hence also the advantage, when the exciting forces are weak or require assistance, of enlarging the mass of the electrolyte; of increasing the size of the electrodes; of making the coppers surround the zincs:—all is in harmony with the view of induction which I am endeavoring to examine; I do not perceive as yet one fact against it.

1353. There are many points of *electrolytic discharge* which ultimately will require to be very closely considered, though I can but slightly touch upon them. It is not that, as far as I have investigated them, they present any contradiction to the view taken (for I have carefully, though unsuccessfully, sought for such cases), but simply want of time as yet to pursue the inquiry, which prevents me from entering upon them here.

1354. One point is, that different electrolytes or dielectrics require different initial intensities for their decomposition (912). This may depend upon the degree of polarization which the particles require before electrolytic discharge commences. It is in direct relation to the chemical affinity of the substances concerned; and will probably be found to have a relation or analogy to the specific inductive capacity of different bodies (1252. 1296.). It thus promises to assist in causing the great truths of those extensive sciences, which are occupied in considering the forces of the particles of matter, to fall into much closer order and arrangement than they have heretofore presented.

1355. Another point is, the facilitation of electrolytic conducting power or discharge by the addition of substances to the dielectric employed. This effect is strikingly shewn where water is the body whose qualities are improved, but, as yet, no general law governing all the phenomena has been detected. Thus some acids, as the sulphuric, phosphoric, oxalic, and nitric, increase the power of water enormously; whilst others, as the tartaric and citric acids, give but little power; and others, again, as the acetic and boracic acids, do not produce a change sensible to the voltameter (739.). Ammonia produces no effect, but its carbonate does. The caustic alkalis and their carbonates produce a fair effect. Sulphate of soda, nitre (753.), and many soluble salts produce much effect. Percyanide of mercury and corrosive sublimate produce no effect; nor does iodine, gum, or sugar, the test being a voltameter. In many cases the added substance is acted on either directly or indirectly, and then the phenomena are more complicated; such substances are muriatic acid (758.), the soluble protochlorides, (766.), and iodides (769.), nitric acid (752.), &c. In other cases the substance added is not,

when alone, subject to, or, a conductor of the powers of the voltaic battery, and yet both gives and receives power when associated with water. M. de la Rive has pointed this result out in sulphurous acid,* iodine and bromine†; the chloride of arsenic produces the same effect. A far more striking case, however, is presented by that very influential body sulphuric acid (681.), and probably phosphoric acid also is in the same peculiar relation.

1356. It would seem in the cases of those bodies which suffer no change themselves, as sulphuric acid (and perhaps in all), that they affect water in its conducting power only as an electrolyte; for whether little or much improved, the decomposition is proportionate to the quantity of electricity passing (727. 730.), and the transfer is therefore due to electrolytic discharge. This is in accordance with the fact already stated as regards water (984.)[‡] that the conducting power is not improved for electricity of force below the electrolytic intensity of the substance acting as the dielectric; but both facts (and some others) are against the opinion which I formerly gave, that the power of salts, &c, might depend upon their assumption of the liquid state by solution in the water employed (410.). It occurs to me that the effect may perhaps be related to, and have its explanation in differences of specific inductive capacities.

1357. I have described in the last paper, cases, where shell-lac was rendered a conductor by absorption for ammonia (1294.). The same effect happens with muriatic acid; yet both these substances, when gaseous, are non-conductors; and the ammonia, also when in strong solution (748.). Mr. Harris has mentioned instances§ in which the conducting power of metals is seriously altered by a very little alloy. These may have no relation to the former cases, but nevertheless should not be overlooked in the general investigation which the whole question requires.

1358. Nothing is perhaps more striking in that class of dielectrics which we call electrolytes, than the extraordinary and almost complete suspension of their peculiar mode of effecting discharge when they are rendered *solid* (380, &c.), even though the intensity of the induction acting through them may be increased a hundred fold or more (419.). It not only establishes a very general relation between the physical properties of these bodies and electricity acting by

* Quarterly Journal, xxvii. 407. or Bibliotheque Universelle, xl. 205. Kemp says sulphurous acid is a very good conductor, Quarterly Journal, 1831, p. 613.

† Quarterly Journal, xxiv. 465, or Annales de Chimie, xxxv. 161.

‡ Philosophical Transactions, 1827, p. 22.

induction through them, but draws both their physical and chemical relations so near together, as to make us hope we shall shortly arrive at the full comprehension of the influence they mutually possess over each other.

¶ ix *Disruptive discharge and insulation.*

1359. The next form of discharge has been distinguished by the adjective *disruptive* (1319.), as it in every case displaces more or less the particles amongst and across which it suddenly breaks. I include under it, discharge in the form of sparks, brushes and glow (1405.), but exclude the cases of currents of air, fluids &c, which, though frequently accompanying the former, are essentially distinct in their nature.

1360. The conditions requisite for the production of an electric spark in its simplest form are well known. An insulating dielectric must be interposed between two conducting surfaces in opposite states of electricity, and then if the actions be continually increased in strength, or otherwise favored, either by exalting the electric state of the two conductors, or bringing them nearer to each other, or diminishing the density of the dielectric, a *spark* at last appears, and the two forces are for the time annihilated, for *discharge* has occurred.

1361. The conductors (which may be considered as the termini of the inductive action) are in ordinary cases most generally metals, whilst the dielectrics usually employed are common air and glass. In my view of induction, however, every dielectric becomes of importance, for as the results are considered essentially dependent on these bodies, it was to be expected that differences of action, never before suspected, would be evident upon close examination, and so at once give fresh confirmation of the theory, and open new doors of discovery into the extensive and varied fields of our science. This hope was especially entertained with respect to the gases, because of their high degree of insulation, their uniformity in physical condition, and great difference in chemical properties.

1362. All the effects prior to the discharge are inductive; and the degree of tension which it is necessary to attain before the spark passes is, therefore, in the examination I am now making of the new view of induction, a very important point. It is the limit of the influence which the dielectric exerts in resisting discharge; it is a measure, consequently, of the conservative power of the dielectric, which in its turn may be considered as becoming a measure, and therefore a representative of the intensity of the electric forces in activity.

1363. Many philosophers have examined the circumstances

of this limiting action in air, but, as far as I know, none have come near Mr. Harris as to the accuracy with, and the extent to, which he has carried on his investigations.* Some of his results I must very briefly notice, premising that they are all obtained with the use of air as the *dielectric* between the conducting surfaces.

1364. First as to the *distance* between the two balls used, or in other words, the *thickness* of the dielectric across which the induction was sustained. The quantity of electricity, measured by a unit jar or otherwise on the same principle with the unit jar, in the charged or inductive ball, necessary to produce spark discharge, was found to vary exactly with the distance between the balls, or between the discharging points, and that under very varied and exact forms of experiment.†

1365. Then with respect to variation in the *pressure or density* of the air. The quantities of electricity required to produce discharge across a *constant* interval varied exactly with variations of the density; the quantity of electricity and density of the air being in the same simple ratio. Or, if the quantity was retained the same, whilst the interval and the density of the air were varied, then these were found in the inverse simple ratio of each other, the same quantity passing across twice the distance with air rarefied to one half.‡

1366. It must be remembered that these effects take place without any variation of the inductive force by condensation or rarefaction of the air. That force remains the same in air,§ and in all gases (1284. 1292), whatever their rarefaction may be.

1367. Variation of the *temperature* of the air produced no variation of the quantity of electricity required to cause discharge across a given interval.||

Such are the general results, which I have occasion for at present, obtained by Mr. Harris, and they appear to me to be unexceptionable.

1368. In the theory of induction founded upon a molecular action of the dielectric, we have to look at the state of that body principally for the cause and determination of the above effects. Whilst the induction continues, it is assumed that the particles of the dielectric are in a certain polarized state, the tension of this state rising higher in each particle as the induction is raised to a higher degree, either by approximation of the inducing surfaces, variations of form, increase of

* Philosophical Transactions, 1834, p. 225.

§ Ibid. pp. 237, 244.

|| Ibid. p. 230.

† Ibid.

‡ Ibid. p. 229.

the original force, or other means; until at last, the tension of the particles having reached the utmost degree which they can sustain without subversion of the whole arrangement, discharge immediately after takes place.

1369. The theory does not assume, however, that *all* the particles of the dielectric subject to the inductive action are affected to the same amount, or acquire the same tension. What has been called the lateral action of the lines of inductive force (1231. 1297.), and the diverging and occasionally curved form of these lines is against such a notion. The idea is, that any section taken through the dielectric across the lines of inductive force, and including *all of them*, would be equal, in the sum of the forces, to the sum of the forces in any other section; and that, therefore, the whole amount of tension for each such section would be the same.

1370. Discharge probably occurs, not when all the particles have attained to a certain degree of tension, but when that particle which is most affected has been exalted to the subverting or turning point (1410.). For though *all* the particles in the line of induction resist charge, and are associated in their actions so as to give a sum of resisting force, yet when any one is brought up to the overturning point, *all* must give way in the case of a spark between ball and ball. The breaking down of that one must of necessity cause the whole barrier to be overturned, for it was at its utmost degree of resistance when it possessed the aiding power of that one particle, in addition to the power of the rest, and the power of that one is now lost. Hence *tension* or *intensity** may, according to the theory, be considered as represented by the particular condition of the particles, or the amount in them of forced variation from their normal state (1298. 1368.)

1371. The whole effect produced by a charged conductor on a distant conductor, insulated or not, is by my theory assumed to be due to an action propagated from particle to particle of the intervening and insulating dielectric, the particles being considered as thrown for the time being into a forced condition, from which they endeavor to return to their normal or natural state. The theory, therefore, seems to supply an easy explanation of the influence of *distance* in affecting induction (1303. 1364.). As the distance is diminished induction increases; for there are then fewer particles in the line of inductive force to oppose their resistance to the assumption of the forced or polarized state, and *vice versâ*. Again, as the

* See Harris on proposed particular meaning of these terms, Philosophical Transactions, 1831, p. 222.

distance diminishes, discharge across happens with a lower charge of electricity; for if, as in Harris's experiments (1364.), the interval be diminished to one half, then half the electricity required to discharge across the first interval is sufficient to strike across the second; and it is evident, also, that at that time there are only half the number of interposed molecules uniting their forces to resist the discharge.

1372. The effect of enlarging the conducting surfaces which are opposed to each other in the act of induction, is, if the electricity be limited in its supply, to lower the intensity of action; and this follows as a very natural consequence from the increased area of the dielectric across which the induction is effected. For by diffusing the inductive action, which at first was exerted through one square inch of sectional area of the dielectric, over two or three square inches of such area, twice or three times the number of molecules of the dielectric are brought into a polarized condition, and employed in sustaining the inductive action, and consequently the tension belonging to the smaller number on which the limited force was originally accumulated, must fall in a proportionate degree.

1373. For the same reason diminishing these opposing surfaces must increase the intensity up to the condition even of their becoming points. But in this case, the tension of the particles of the dielectric next the points is higher than that of particles midway, because of the lateral action and consequent bulging, as it were, of the lines of inductive force at the middle distance (1369.).

1374. The more exalted effects of induction on a point *p*, or any small surface, as the rounded end of a rod, opposed to a large surface, as that of a ball or plate, than when it is opposed to another point or end at the same distance, falls into harmonious relation (1302.). For in the latter case, the small surface *p* is effected only by those particles which are brought into the inductive condition by the equally small surface of the opposed conductor, whereas when that is a ball or plate the lines of inductive force from the latter are concentrated, as it were, upon the end *p*. Now though the molecules of the dielectric against the large surface may have a much lower state of tension than those against the similar smaller surface, yet they are also far more numerous, and, as the lines of inductive force converge towards a point, are able to communicate to the particles contained in any cross section (1369.) nearer the small surface an amount of tension equal to their own, and consequently much higher for each individual particle; so that, at the surface of the smaller conductor, the tension of a particle rises much, and if that conductor were to terminate in a

point, the tension would rise to an infinite degree, except that it is limited, as before (1368.), by discharge. The nature of the discharge from small surfaces and points under induction will be resumed hereafter (1425. &c.).

1375. *Rarefaction* of the air does not alter the *intensity* of inductive action (1284. 1287.); nor is there any reason, as far as I can perceive, why it should. If the quantity of electricity and the distance remain the same, and the air be rarefied one half, then, though one half of the particles of the dielectric are removed, the other half assume a double degree of tension in their polarity, and therefore the inductive forces are balanced, and the result remains unaltered as long as the induction and insulation are sustained. But the case of *discharge* is very different; for as there are only half the number of dielectric particles in the rarefied atmosphere, so these are brought up to the discharging intensity by half the former quantity of electricity; discharge, therefore, ensues, and such a consequence of the theory is in perfect accordance with Mr. Harris's results (1365.).

1376. The *increase* of electricity required to cause discharge over the same distance, when the pressure of the air or its density is increased, flows in a similar manner, and on the same principle, from the molecular theory.

1377. Here I think my view of induction has a decided advantage over others, especially over that which refers the retention of electricity on the surface of conductors in air to the *pressure of the atmosphere*. The latter is the view which, being adopted by Poisson and Biot,* is also, I believe, that generally received; and it relates two such dissimilar things, as the ponderous air and the subtil and even hypothetical fluid or fluids of electricity, by gross mechanical relations; by the bonds of mere static pressure. My theory, on the contrary, sets out at once by connecting the electric forces with the particles of matter; it derives all its proofs, and even its origin in the first instance, from experiment; and then, without any further assumption, seems to offer at once a full explanation of these and many other singular, peculiar, and, I think, heretofore unrelated effects.

1378. An important assisting experimental argument may here be adduced, derived from the difference of specific inductive capacity of different dielectrics (1269. 1274. 1278.). Consider an insulated sphere electrified positively and placed in the centre of another and larger sphere uninsulated, a uniform dielectric, as air, intervening. The case is really that

* Ency. Britann. Supplement, vol. iv. Article Electricity, pp. 76, 81, &c.

of my apparatus (1187.), and also, in effect, that of any ball electrified in a room and removed to some distance from irregularly formed conductors. Whilst things remain in this state the electricity is distributed (so to speak) uniformly over the surface of the electrified sphere. But introduce such a dielectric as sulphur or lac, into the space between the two conductors on one side only, or opposite one part of the inner sphere, and immediately the electricity on the latter is diffused unequally (1229. 1270. 1309.), although the form of the conducting surfaces, their distances, and the *pressure* of the atmosphere remain perfectly unchanged.

1379. Fusinieri took a different view from that of Poisson, Biot, and others, of the reason why rarefaction of air caused easy diffusion of electricity. He considered the effect as due to the removal of the *obstacle* which the air presented to the expansion of the substances from which the electricity passed.* But platina balls shew the phenomena in vacuo as well as volatile metals and other substances; besides which, when the rarefaction is very considerable, the electricity passes with scarcely any resistance, and the production of no sensible heat; so that I think Fusinieri's view of the matter is likely to gain but few assents.

1380. I have no need to remark upon the discharging or collecting power of flame or hot air. I believe, with Harris, that the mere heat does nothing (1367.), the rarefaction only being influential. The effect of rarefaction has been already considered generally (1375.); and that caused by the heat of a burning light, with the pointed form of the wick, and the carrying power of the carbonaceous particles which for the time are associated with it, are fully sufficient to account for all the effects.

1381. We have now arrived at the important question, how will the inductive tension requisite for insulation and disruptive discharge be sustained in gases, which, having the same physical state and also the *same pressure* and the *same temperature* as air, differ from it in specific gravity, in chemical qualities, and it may be in peculiar relations, which not being as yet recognised, are purely electrical (1361.)?

1382. Into this question I can enter now only as far as is essential for the present argument, namely, that insulation and inductive tension do not depend merely upon the charged conductors employed, but also, and essentially, upon the inter-

* Bib. Univ. 1831. xlviii. 375.

posed dielectric, in consequence of the molecular action of its particles.

1383. A glass vessel *a* (fig. 13.)* was ground at the top and bottom so as to be closed by two ground brass plates, *b* and *c*; *b* carried a stuffing box, with a sliding rod *d* terminated by a brass ball *s* below, and a ring above. The lower plate was connected with a foot, stop-cock, and socket, *e*, *f* and *g*; and also with a brass ball *l*, which by means of a stem attached to it and entering the socket *g*, could be fixed at various heights. The metallic parts of this apparatus were not varnished, but the glass was well covered with a coat of shell-lac previously dissolved in alcohol. On exhausting the vessel at the air-pump, it could be filled with any other gas than air, and, in such cases, the gas so passed in was dried whilst entering by fused chloride of calcium.

1384. The other part of the apparatus consisted of two insulating pillars, *h* and *i*, to which were fixed two brass balls, and through these passed two sliding rods, *k* and *m*, terminated at each end by brass balls; *n* is the end of an insulated conductor, which could be rendered either positive or negative from an electrical machine; *o* and *p* are wires connecting it with the two parts previously described, and *q* is a wire which, connecting the two opposite sides of the collateral arrangements, also communicates with a good discharging train *r* (292.).

1385. It is evident that the discharge from the machine electricity may pass either between *s* and *l*, or *S* and *L*. The regulation adopted in the first experiments was to keep *s* and *l* with their distance *unchanged*, but to introduce first one gas and then another into the vessel *a*, and then balance the discharge at the one place against that at the other; for by making the interval at *u* sufficiently small, all the discharge would pass there, or making it sufficiently large it would all occur at the interval *v* in the receiver. On principle it seemed evident, that in this way the varying interval *u* might be taken as a measure, or rather indication of the resistance to discharge through the gas at the constant interval *v*. The following are the constant dimensions.

Ball <i>s</i>	0.93 of an inch.
Ball <i>S</i>	0.96 of an inch.
Ball <i>l</i>	2.02 of an inch.
Ball <i>L</i>	1.95 of an inch.
Interval <i>v</i>	0.62 of an inch.

1386. On proceeding to experiment it was found that when air or any gas was in the receiver *a*, the interval *u* was not a fixed

* The drawing is to a scale of 1.6.

one; it might be altered through a certain range of distance, and yet sparks pass either there or at *v* in the receiver. The extremes were therefore noted, i. e., the greatest distance short of that at which the discharge *always* took place at *v* in the gas, and the least distance short of that at which it *always* took place at *u* in the air. Thus, with air in the receiver, the extremes at *u* were 0.56 and 0.79 of an inch, the range of 0.23 being one at which sparks passed occasionally either at one interval or the other.

1387. The small balls *s* and *S* could be rendered either positive or negative from the machine, and as gases were expected and were found to differ from each other in relation to this change, the results obtained under these differences of charge were also noted.

1388. The following is a table of results; the gas named is that in the vessel *a*. The smallest, greatest, and mean interval at *u* in air is expressed in parts of an inch, the interval *v* being constantly 0.62 of an inch.

	Smallest.	Greatest.	Mean.
{ Air, <i>s</i> and <i>S</i> , pos.	0.60	0.79	0.695
{ Air, <i>s</i> and <i>S</i> , neg.	0.59	0.68	0.635
{ Oxygen, <i>s</i> and <i>S</i> , pos.	0.41	0.60	0.505
{ Oxygen, <i>s</i> and <i>S</i> , neg.	0.50	0.52	0.510
{ Nitrogen, <i>s</i> and <i>S</i> , pos.	0.55	0.68	0.615
{ Nitrogen, <i>s</i> and <i>S</i> , neg.	0.59	0.70	0.645
{ Hydrogen, <i>s</i> and <i>S</i> pos.	0.30	0.44	0.370
{ Hydrogen, <i>s</i> and <i>S</i> neg.	0.25	0.30	0.275
{ Carbonic acid, <i>s</i> and <i>S</i> pos.	0.56	0.72	0.640
{ Carbonic acid, <i>s</i> and <i>S</i> neg.	0.58	0.60	0.590
{ Olefiant gas, <i>s</i> and <i>S</i> pos.	0.64	0.86	0.750
{ Olefiant gas, <i>s</i> and <i>S</i> neg.	0.69	0.77	0.730
{ Coal gas, <i>s</i> and <i>S</i> pos.	0.37	0.61	0.490
{ Coal gas, <i>s</i> and <i>S</i> neg.	0.47	0.58	0.525
{ Muriatic acid gas, <i>s</i> and <i>S</i> pos.	0.89	1.32	1.105
{ Muriatic acid gas, <i>s</i> and <i>S</i> neg.	0.67	0.75	0.720

1389. The above results were all obtained at one time. On other occasions other experiments were made, which gave generally the same results as to order, though not as to numbers. Thus :

Hydrogen, <i>s</i> and <i>S</i> pos.	0.23	0.57	0.400
Carbonic acid, <i>s</i> and <i>S</i> pos.	0.51	1.05	0.780
Olefiant gas, <i>s</i> and <i>S</i> pos.	0.66	1.27	0.965

I did not notice the difference of the barometer on the days of experiment.

1390. One would have expected only two distances, one for each interval, for which the discharge might happen either at one or the other; and that the least alteration of either would immediately cause one to predominate constantly over the other. But that under common circumstances is not the case. With air in the receiver, the variation amounted to 0.2 of an inch nearly on the smaller interval of 0.6, and with muriatic acid gas, the variation was above 0.4 on the smaller interval of 0.9. Why is it that when a fixed interval (the one in the receiver) will pass a spark that cannot go across 0.6 of air at one time, it will immediately after, and apparently under exactly similar circumstances, not pass a spark that can go across 0.8 of air?

1391. It is probable that part of this variation will be traced to particles of dust in the air drawn into and about the circuit. I believe also that part depends upon a variable charged condition of the surface of the glass vessel *a*. That the whole of the effect is not traceable to the influence of circumstances in the vessel *a*, may be deduced from the fact, that when sparks occur between balls in free air they frequently are not straight, and often pass otherwise than by the shortest distance. These variations in air itself, and at different parts of the very same balls, shew the presence and influence of circumstances which are calculated to produce effects of the kind now under consideration.

(*To be Continued.*)

XIII.—*Experiments relative to the propagation of Caloric in a Metallic Bar.* By M. H. SCHROEDER, Esq. Professor of Natural Philosophy at Soleure. Communicated by the Author.

It is well known that artificers in metal, advance generally enough the curious fact, that a metallic bar one end of which is placed in an ardent furnace, and held by the other end until the heat has begun to be sensibly felt by the hand, will suddenly cause a very considerable elevation of temperature to be felt, immediately on removing out of the fire the heated extremity of the bar, in order to expose it to the cold air, or even to plunge it in cold water. I know of no other experiments on the subject than those of M. Fischer, of Breslaw, (*Proggend. Ann.* 19,—507), and those of Professor Mousson,

of Zurich, who communicated them last year in the section of Natural Philosophy, at the meeting of Swiss Naturalists, held at Neufchatel. These two philosophers seem not to doubt of the existence of the fact in question, but the former is or seems willing to attribute it to a variation in the conducting faculty of metals according to their temperature, whilst M. Mousson considers it as proving a developement of specific caloric, produced by a molecular compression which would be caused by the sudden cooling to which the metal was subjected.

Having several reasons to doubt whether the truth of the fact or phenomena had been rigorously established by experiments made up to the present time, I undertook to convince myself, by decisive and direct experiments, whether the phenomena had existed or not, and in the latter case, to shew that a false explanation of other phenomena on the part of these philosophers, and a common illusion on the part of the artisans, are the only reasons for advancing a position so paradoxical. Now such happens to be the result at which I have arrived in making use of the means offered to us by thermo-electric forces of measuring the changes of temperature; and I have given to the experiments which I am about to describe, another importance besides that of having served to demonstrate in a decisive manner, that the phenomena now in question has no existence whatever.

I obtained a galvanometer of a very delicate and sensitive construction used in measuring thermo-electric currents, on the model of that which Fechner has recommended as most convenient for that purpose. It consists of a large band of copper, folded once round an astatic system consisting of two magnetized needles,* which could oscillate freely on a graduated circle from degree to degree. A simple element of bismuth and antimony, communicating with the reservoirs of mercury, into which the ends of the band of copper are plunged, gives to the needle a continued deviation from 80 to 85 degrees, on applying solely the temperature of the hand to the point of the solder. I afterwards soldered even at the soldering point of the element of bismuth and antimony, one of the extremities of a bar of another metal, long enough to allow the free extremity to be exposed to the action of heat, without any direct influence being exercised on the solder of the section of bismuth and antimony, then keeping the eye invariably fixed on the needle, I awaited the

* No upper needle ought to be *above* the upper side of the coil, and the lower needle within the coil.—EDIT.

time when the temperature of the bar would become permanent throughout its whole extent, which was easily observable by a constant deviation of the galvanometer. Immediately on the arrival of this moment, I removed, by an assistant, the source of heat, and plunged the red hot extremity into cold water. If the sudden cooling of the red hot extremity of the bar had power to produce in any manner whatever an augmentation of the temperature of the other extremity, it is clear that this heat would be communicated to the point of the solder, and that the needle, before making a retrograde movement, which would be the effect of the cooling having commenced, ought first to manifest an instantaneous deviation, in the contrary direction, thereby indicating an augmentation of temperature. But what was the change in the circumstances of this experiment? never have I been able to perceive the least sign of an effect approaching to this. The needle, on the contrary, remained each trial, immovable during some time, until perhaps the cooling had been communicated to the point of the solder in the ordinary manner, and then began immediately to indicate the direct effect of cooling. I have repeated these experiments, under very different circumstances with bands, with bars, with wires, with masses of different forms both of iron, of copper, of zinc, of brass, &c. giving them different lengths from two feet to half an inch; making use of sources of heat of a temperature more or less elevated; producing the sudden cooling sometimes by the action of the air, sometimes by water, and at others by mercury; and in making use of the thermo-electric element, a combination of any two metals; but in no case did the needle indicate to me an augmentation of temperature after the the operation of cooling. The result of these experiments being absolutely negative, I judge it not necessary to describe them in detail; but I conclude therefrom with certainty that the fact in question does not exist.

This point once established, it remained for me to repeat conveniently the experiments of M. Mousson, in order to give the true explanation to the phenomena which he has observed. Now I am convinced, by a careful examination, that the explanation of them coincides with that of a well known phenomenon, which may be observed easily by any thermometer in any degree sensitive, namely, that if the instrument be suddenly withdrawn from the source of heat, at the *first* moment there will be seen a rapid elevation, after which the depression will commence. We know that this circumstance is owing to the sudden contraction of the small bowl of glass, and to its diminution of volume, which surpasses during the

first moments the effect of the depression of temperature on the whole column of mercury. The phenomena which M. Mousson has observed are quite analogous to this, and it appears to me that he has deduced consequences from it which cannot be admitted or maintained.

Nevertheless the hypothesis which he has advanced, that a sudden cooling ought to cause a molecular compression, which in itself ought to become the means of developement of specific caloric, appears to me to be admissible in a particular case, that is to say, when it acts on the *interior* parts of a mass heated throughout its whole extent, at the first moment wherein the *exterior* surface is cooled with sufficient rapidity.

I hope however, to be able to demonstrate this fact by experiments. In this instance, I took a small cylinder of iron, in the upper side of which I inserted a spiral iron wire sufficiently strong for the purpose. I covered this cylinder up to half its height in a crucible, leaving the principal points bare, then I soldered a copper wire of nearly two lines diameter, in such a manner that the copper did not touch the iron except at the point of the solder. I afterwards fixed the ends of the iron wire and the copper wire in the reservoirs of mercury, which communicated with the band of the galvanometer, and then I heated the cylinder by the heat of spirits of wine. I expected that the exterior parts of the mass (if it is true that in these circumstances they are contracted suddenly by the effect of a depression of temperature,) would exercise an instantaneous compression of the interior parts, which would give place to a developement of specific caloric, which would be indicated by the galvanometer. In order to find the most favorable circumstances to obtain exactness in the experiment, I observed at first the successive positions of the needle during the time of heating. The needle commenced by deviating to 72° ; then, the heating still going forward, not only did it return with great rapidity to the point 0° of the division, but passed it, making a contrary or negative deviation nearly to 65° . The continuance of the process of heating produced then a contrary thermo-electric relation between the iron and the copper, inasmuch as the direction of the current depends on the temperature of the point of the solder.

I profited by this remark, and now put the lamp at such a distance from the mass of iron, that the needle had power to take nearly a constant position at the point 0° of the division whenever the temperature of the mass became permanent. This I easily accomplished after one or two trials. In, fact, in this position the needle had the greatest sensibility for the

least change of temperature, and it sufficed, on approaching it to the flame for an instant, to make it deviate for some seconds after with a quick movement, almost like a shake towards the negative side of the point 0° , on such a manner, that it could not retain its constant position until after several oscillations. The iron was at that point of temperature approaching to a red heat. I fixed my eye on the needle, and caused the inferior portion of the mass of iron to be plunged into cold mercury. But the needle remained immovable during the first moments, then immediately afterwards indicated to me a prompt slackening of the heat or cooling from the point of the solder. This effect was very contrary to that which I had expected, because it appeared to me, that a strong molecular compression ought necessarily to take place in this case. I do not wish to conclude from this; that a developement of specific heat, caused by a due compression by contraction of those parts suddenly cooled, has not taken place in any case; but we see however, that if the fact does exist, it ought at least to be of very little importance, for that it cannot be perceived even under the most favorable circumstances, with an instrument so sensitive as the galvanometer of which I have spoken. It is very necessary then to be able to make use of and to explain phenomena so confidently asserted as those described by M. Mousson. I have named this last experiment because it appeared to me to have given a result contrary to what we should have expected.

I have besides these made some experiments relative to those which M. Fischer has published. But as these are not independent of the sensation of the hand, I think we ought not to attribute any great importance to them; I content myself then simply with saying, that my experiments have conducted me to the belief that M. F. has partaken of the same illusion, to which artificers in metal, ordinarily abandon themselves.

XIV.—*On a New Electro-Magnetic Engine.* By THOMAS WRIGHT, Esq.

Dear Sir,

I see in your last number an account of a new Electro-magnetic Engine by Mr. Clarke, in which he has dispensed with the rotary motion, and returned to the crank. I have been lately engaged in some experiments, the results of which, incline me to believe that this will be the best form of applying the electro-magnetic power. I adopt however, a very different arrangement from that of Mr. Clarke.

The magnetism induced in soft iron by electric currents has great sustentive, but small attractive power, in comparison with that of steel magnets; the great power which the former possess not being developed, until their keepers are in connexion with their poles. In Taylor's rotatory engine, which I have reason to think is the best, the magnets are passed by their keepers with such rapidity, that I should think it impossible that their power can be developed, as it requires a certain length of time for that purpose.

The great difficulty in the construction of electro-magnetic engines with the crank motion, is the shortness of the stroke, which, for any useful purposes, should not exceed a quarter of an inch. I have, however, been able to increase the length of the stroke materially, by two methods:—the first consists in keeping one end of the armature on its respective pole, as at *a* fig. 1 plate 3, and taking the stroke between B and C; thus interrupting the magnetic circuit at one place only; this plan answers better with small electro-magnets, which generally possess greater comparative intensity, than those of a largesize. A small electro-magnet, a quarter of an inch square, and seven inches long, coiled with ten yards of copper bell-wire, attracted its keeper, placed parallel with its poles, *a quarter of an inch*, (along a polished mahogany table,) when, however, the keeper was placed as in fig. 1, it attracted the other end *an inch and a quarter*; this was the result of several experiments. The second method which is better adapted for large electro-magnets, consists in keeping one *side* of the armature on the magnet as in fig. 2; this I think will be the most available method of applying the power, and I am now making a small engine on this principle, on which I hope to have your opinion in a short time.

The magnets employed in this engine are straight and composed of hoop iron, well annealed and riveted together, and are coiled to within an inch and a half from the ends, to which are riveted pieces of soft iron two inches square, as shewn at A fig. 3, plate 3; these magnets are then united in pairs, by having the pieces of soft iron attached to them hinged together at B, they are thus brought as close as possible to each other along their whole length.

Fig. 4, is a plan of the engine with two pairs of magnets, the end of the lower magnet of each pair being concealed by the pillar on which it rests; the armature of the upper magnet of the pair on the right hand, forms part of the beam of the engine, the hinge which attaches it to the lower armature being the fulcrum. The upper armature of the other pair is attached to the beam by a rod at A; it will thus be seen that

by making communication between the battery and each pair of magnets alternately, the armatures of the magnetized pair will be strongly attracted to each other, lifting up the armature of the other pair respectively, and working the fly wheel.

B B B, fig. 4 plate, shews the apparatus on the shaft of the fly wheel, for throwing the battery power alternately on the pairs of magnets.

I am, my dear Sir,

Yours truly,

THOMAS WRIGHT.

XV.—*An Answer to Dr. Hare's Letter on certain Theoretical Opinions.* By M. Faraday, Esq. F. R. S.

My dear Sir,

1. Your kind remarks have caused me very carefully to revise the general principles of the view of *static induction* which I have ventured to put forth, with the very natural fear that as it did not obtain your acceptance, it might be founded in error; for it is not a mere complimentary expression when I say I have very great respect for your judgment. As the reconsideration of them has not made me aware that they differ amongst themselves or with facts, the resulting impression on my mind is, that I must have expressed my meaning imperfectly, and I have a hope that when more clearly stated my words may gain your approbation. I feel that many of the words in the language of electrical science possess much meaning; and yet their interpretation by different philosophers often varies more or less, so that they do not carry exactly the same idea to the minds of different men: this often renders it difficult, when such words force themselves into use, to express with brevity as much as, and no more than, one really wishes to say.

2. My theory of induction (as set forth in Series xi., xii., and xiii.,) makes no assertion as to the nature of electricity, or at all questions any of the theories respecting that subject (1667). It does not even include the origination of the developed or excited state of the power or powers; but taking that as it is given by experiment and observation, it concerns itself only with the arrangement of the force in its communication to a distance in that particular yet very general phenomenon called *static induction* (1668.). It is neither the nature nor the amount of the force which it decides upon, but solely its mode of distribution.

3. Bodies whether conductors or non-conductors can be *charged*. The word *charge* is equivocal: sometimes it means that state which a glass tube acquires when rubbed by silk, or

which the prime conductor of a machine requires when the latter is in action ; at other times it means the state of a Leyden jar or a similar inductive arrangement when it is said to be charged. In the first case the word means only the peculiar condition of an electrified mass of matter considered by itself, and does not apparently involve the idea of induction ; in the second it means the whole of the relations of two such masses charged in opposite states, and most intimately connected by inductive action.

4. Let three insulated metallic spheres, A, B, and C, be placed in a line, and not in contact ; let A be electrified positively, and then C uninsulated ; besides the general action of the whole system upon all surrounding matter, there will occur a case of inductive action amongst the three balls, which may be considered apart, as the type and illustration of the whole of my theory : A will be charged positively ; B will acquire the negative state at the surface towards A, and the positive state at the surface furthest from it ; and C will be charged negatively.

5. The ball B will be in what is often called a polarized condition, i. e. opposite parts will exhibit the opposite electrical states, and the two sums of these opposite states will be exactly equal to each other. A and C will not be in this polarized state, for they will each be, as it is said, charged (3), the one positively, the other negatively, and they will present no polarity as far as this particular act of induction (4) is concerned.

6. That one part of A is more positive than another part does not render it polar in the sense in which that word has just been used. We are considering a particular case of induction, and have to throw out of view the states of those parts not under the inductive action. Or if any embarrassment still arise from the fact that A is not uniformly charged all over, then we have merely to surround it with balls, such as B and C, on every side, so that its state shall be alike on every part of its surface (because of the uniformity of its inductive influence in all directions) and then that difficulty will be removed. A therefore is charged, but not polarly ; B assumes a polar condition ; and C is charged inducteously (1483), being by the prime influence of A brought into the opposite or negative electrical state through the intervention of the intermediate and polarized ball B.

7. Simple charge therefore does not imply polarity in the body charged. Inductive charge (applying that term to the sphere B and all bodies in a similar condition (5) does (1672.). The word charge as applied to a Leyden jar, or to the *whole*

of any inductive arrangement, by including *all* the effects, comprehends of course both these states.

8. As another expression of my theory, I will put the following the case. Suppose a metallic sphere C, formed of a thin shell a foot in diameter; suppose also in the centre of it another metallic sphere A only an inch in diameter; suppose the central sphere A charged positively with electricity to the amount we will say of 100; it would act by induction through the air, lac, or other insulator between it and the large sphere C; the interior of the latter would be negative, and its exterior positive, and the sum of the positive force upon the whole of the external surface would be 100. The sphere C would in fact be polarized (5) as regards its inner and outer surfaces.

9. Let us now conceive that instead of mere air, or other insulating dielectric, within C between it and A, there is a thin metallic concentric sphere B six inches in diameter. This will make no difference in the ultimate result, for the charged ball A will render the inner and outer surfaces of this sphere B negative and positive, and it again will render the inner and outer surfaces of the large sphere C negative and positive, the sum of the positive forces on the outside of C being still 100.

10. Instead of one intervening sphere let us imagine 100 or 1000 concentric with each other, and separated by insulating matter, still the same final result will occur; the central ball will act inductrically, the influence originating with it will be carried on from sphere to sphere, and positive force equal to 100 will appear on the outside of the external sphere.

11. Again, imagine that all these spheres are subdivided into myriads of particles, each being effectively insulated from its neighbours (1679.), still the same final result will occur; the inductric body A will polarize all these, and having its influence carried on by them in their newly acquired state, will exert precisely the same amount of action on the external sphere C as before, and positive force equal to 100 will appear on its outer surface.

12. Such a state of the space between the inductric and inducteous surfaces represents what I believe to be the state of an insulating dielectric under inductive influence; the particles of which by the theory are assumed to be conductors individually, but not to one another (1669.).

13. In asserting that 100 of positive force will appear on the outside of the external sphere under all these variations, I presume I am saying no more than what every electrician will admit. Were it not so, then positive and negative elec-

tricitities could exist by themselves, and without relation to each other (1169. 1177.), or they could exist in proportions not equivalent to each other. There are plenty of experiments, both old and new, which prove the truth of the principle, and I need not go further into it here.

14. Suppose a plane to pass through the centre of this spherical system, and conceive that instead of the space between the central ball A and the external sphere C being occupied by a uniform distribution of the equal metallic particles, three times as many were grouped in the one half to what occurred in the other half, the insulation of the particles being always preserved: then more of the inductive influence of A would be conveyed outwards to the inner surface of the sphere C, through that half of the space where the greater number of metallic particles existed, than through the other half: still the exterior of the outer sphere C would be uniformly charged with positive electricity, the amount of which would be 100 as before.

15. The actions of the two portions of space, as they have just been supposed to be constituted (14), is as if they possessed two different *specific inductive capacities* (1296.); but I by no means intend to say, that *specific inductive capacity* depends in all cases upon the number of conducting particles of which the dielectric is formed, or upon their vicinity. The full cause of the evident difference of inductive capacity of different bodies is a problem as yet to be solved.

16. In my papers I speak of all induction as being dependent on the action of contiguous particles, i. e. I assume that insulating bodies consist of particles which are conductors individually (1669.), but do not conduct to each other provided the intensity of action to which they are subject is beneath a given amount (1326. 1674. 1675.); and that when the inductive body acts upon conductors at a distance, it does so by polarizing (1293. 1670.) all those particles which occur in the portion of dielectric between it and them. I have used the term *contiguous* (1164. 1673.), but have I hope sufficiently expressed the meaning I attach to it: first by saying at par. 1615, "the next existing particle being considered as the contiguous one;" then in a note to par. 1665, by the words, "I mean by contiguous particles those which are next to each other, not that there is no space between them;" and further by the note to par. 1164. of the octavo edition of my *Researches*, which is as follows: "The word contiguous is perhaps not the best that might have been used here and elsewhere, for as particles do not touch each other it is not strictly correct. I was induced to employ it because in its common ac-

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acceptation it enabled me to state the theory plainly and with facility. By contiguous particles, I mean those which are next."

17. Finally, my reasons for adopting the molecular theory of induction were the phenomena of electrolytic discharge (1164. 1343.), of induction in curved lines (1166. 1215.), of specific inductive capacity (1167. 1252.), of penetration and return action (1245.), of difference of conduction and insulation (1320.) of polar forces (1665.), &c. &c., but for these reasons and any strength or value they may possess I refer to the papers themselves.

18. I will now turn to such parts of your critical remarks as may require attention. A man who advances what he thinks to be new truths, and to develop principles which profess to be more consistent with the laws of nature than those already in the field, is liable to be charged, first with self-contradiction; then with the contradiction of facts; or he may be obscure in his expression, and so justly subject to certain queries; or he may be found in non-agreement with the opinions of others. The first and second points are very important, and every one subject to such charges must be anxious to be made aware of, and also to set himself free from or acknowledge them; the third is also a fault to be removed if possible; the fourth is a matter of but small consequence in comparison with the other three; for as every man who has the courage, not to say rashness, of forming an opinion of his own, thinks it better than any from which he differs, so it is only deeper investigation, and most generally future investigators who can decide which is in the right.

19. I am afraid I shall find it rather difficult to refer to your letter. I will, however, reckon the paragraphs in order from the top of each page, considering that the first which has its *beginning* first in the page*. In referring to my own matter I will employ the usual figures for the paragraphs of the Experimental Researches, and small Roman numerals for those of this communication.

20. At par. 3, you say, you cannot reconcile my language at 1615, with that at 1165. In the latter place I have said I believe *ordinary induction* in all cases to be an action of *contiguous* particles, and in the former assuming a very hypothetical case, that of a vacuum, I have said, nothing in my theory forbids that a charged particle in the centre of a vacuum should act on the particle next to it, though that

* We shall change Prof. Faraday's references for the numbers which we have attached to Dr. Hare's letter, and refer thus, par. 23, &c.

should be half an inch off. With the meaning which I have carefully attached to the word *contiguous* (16.), I see no contradiction here in the terms used, nor any natural impossibility or improbability in such an action. Nevertheless all *ordinary* induction is to me an action of contiguous particles, being particles at insensible distances: induction across a vacuum is not an ordinary instance, and yet I do not perceive that it cannot come under the same principles of action.

21. As an illustration of my meaning, I may refer to the case, parallel with mine, as to the extreme difference of interval between the acting particles or bodies, of the modern views of the radiation and conduction of heat. In radiation the rays leave the hot particles and pass occasionally through great distances to the next particle, fitted to receive them: in conduction, where the heat passes from the hotter particles to those which are contiguous and form part of the same mass, still the passage is considered to be by a process precisely like that of radiation; and though the effects are, as is well known, extremely different in their appearance, it cannot as yet be shewn that the principle of communication is not the same in both.

22. So on this point respecting contiguous particles and induction across half an inch of vacuum, I do not see that I am in contradiction with myself or with any natural law or fact.

23. Paragraph 4 is answered by the above remarks, and by 8, 9, and 10.

24. Paragraph 5 is answered according to my theory by 8, 9, 10, 11, 12, and 13.

25. Paragraph 6 is answered, except in the matter of opinion (18.), according to my theory by 16. The conduction of heat referred to in the paragraph itself will, as it appears to me, bear no comparison with the phenomenon of electrical induction:—the first refers to the distant influence of an agent which travels by a very slow process, the second to one where distant influence is simultaneous, so to speak, with the origin of the force at the place of action:—the first refers to an agent which is represented by the idea of one imponderable fluid, the second to an agency better represented probably by the idea of two fluids, or at least by two forces:—the first involves no polar action, nor any of its consequences, the second depends essentially on such actions;—with the first, if a certain portion be originally employed in the centre of a spherical arrangement, but a small part appears ultimately at the surface; with the second, an amount of force appears instantly at the surface (8, 9, 10, 11, 12, 13, and 14.), exactly equal to the exciting or moving force, which is still at the centre.

26. Paragraph 13 involves another charge of self-contradiction, from which, therefore, I will next endeavor to set myself free. You say I "correctly allege that it is impossible to charge a portion of matter with one electric force without the other (see par. 1177.). But if all this be true, how can there be a *positively excited particle*? (see par. 1616.). Must not every particle be excited negatively if it be excited positively? Must it not have a negative as well as a positive pole?" Now I have not said exactly what you attribute to me: my words are, "it is impossible, experimentally, to charge a portion of matter with one electric force *independently* of the other: charge always implies *induction*, for it can in no instance be effected without (1177.)." I can, however, easily perceive how my words have conveyed a very different idea to your mind, and probably to others, than that I meant to express.

27. Using the word *charge* in its simplest meaning (3.4.), I think that a body *can* be charged with one electric force without the other, that body being considered in relation to itself only. But I think that such charge cannot exist without induction (1178.), or independently of what is called the development of an equal amount of the other electric force, not in itself, but in the neighbouring consecutive particles of the surrounding dielectric, and through them of the facing particles of the uninsulated surrounding conducting bodies, which, under the circumstances, terminate as it were the particular case of induction. I have no idea, therefore, that a particle when charged must itself of necessity be polar; the spheres A B C of 4, 5, 6, 7, fully illustrate my views (1672.).

28. Paragraph 20 includes the question, "is this consistent?" implying self-contradiction, which, therefore, I proceed to notice. The question arises out of the possibility of glass being a (slow) conductor or not of electricity, a point questioned also in the two preceding paragraphs. I believe that it is. I have charged small Leyden jars, made of thin flint glass tube, with electricity, taken out the charging wires, sealed them up hermetically, and after two or three years have opened and found no charge in them. I will refer you also to Belli's curious experiments upon the successive charges of a jar and the successive return of portions of these charges.* I will also refer to the experiments with the shell lac hemisphere, especially that described in 1237. of my *Researches*; also the experiment in 1246. I cannot conceive how, in these cases, the air in the vicinity of the coating could gradually relinquish to it a portion of free electricity, conveyed into it by

* *Bibliotheca Italiana*, 1837, lxxxv., p. 417.

what I call convection, since in the first experiment quoted (1237.), when the return was gradual, there was *no coating*; and in the second (1246.), when there was *a coating*, the return action was most sudden and instantaneous.

29. Paragraphs 21 and 22 perhaps only require a few words of explanation. In a charged Leyden jar I have considered the two opposite forces on the inductric and inductive surfaces as being directed towards each other through the glass of the jar, provided the jar have no projection of its inner coating, and is uninsulated on the outside (1682.). When discharge by a wire or discharger, or any other of the many arrangements used for that purpose is effected, these supply the "some other directions" spoken of (1682. 1683.).

30. The inquiry in paragraph 23, I should answer by saying, that the process is the same as that by which the polarity of the sphere B (4. 5.) would be neutralized if the spheres A and C were made to communicate by a metallic wire; or that by which the 100 or 1000 intermediate spheres (10.) or the myriads of polarized conducting particles (11.) would be discharged, if the inner sphere A, and the outer one C, were brought into communication by an insulated wire; a circumstance which would not in the least affect the condition of the power on the exterior of the globe C.

31. The obscurity in my papers, which has led to your remarks in paragraph 25, arises, as it appears to me (after my own imperfect expression), from the uncertain or double meaning of the word discharge. You say, "if discharge involves a return to the same state in vitreous particles, the same must be true in those of the metallic wire. Wherefore then are these dissipated when the discharge is sufficiently powerful?" A jar is said to be discharged when its charged state is reduced by any means, and it is found in its first indifferent condition. The word is then used simply to express the state of the apparatus; and so I have used it in the expressions criticised in paragraph 21, already referred to. The process of discharge, or the mode by which the jar is brought into the discharged state, may be subdivided, as of various kinds; and I have spoken of conductive (1320.), electrolytic (1343.), disruptive (1359.), and convective (1562.) discharge, any one of which may cause the discharge of the jar, or the discharge of the inductive arrangements described in this letter (30), the action of the particles in any one of these cases being entirely different from the mere return action of the polarized particles of the glass jar, or the polarized globe B (5.), to their first state. My view of the relation of insulators and conductors, as bodies of one class), is given at 1320. 1675. &c.

of the Researches: but I do not think the particles of the good conductors acquire an intensity of polarization any thing like that of the particles of bad conductors; on the contrary, I conceive that the contiguous polarized particles (1670.) of good conductors discharge to each other when their polarity is at a very low degree of intensity (1326. 1338. 1675.). The question of why are the metallic particles dissipated when the charge is sufficiently powerful, is one that my theory is not called upon at present to answer, since it will be acknowledged by all, that the dissipation is not necessary to discharge. That different effects ensue upon the subjection of bodies to different degrees of the same power, is common enough in experimental philosophy; thus, one degree of heat will merely make water hot, whilst a higher degree will *dissipate* it as steam, and a lower will convert it into ice.

32. The next most important point, as it appears to me, is that contained in paragraphs 16 and 17. I have said (1330.), "what then is to separate the principle of these two extremes, perfect conduction and perfect insulation, from each other, since the moment we leave in the smallest degree perfection at either extremity we involve the element of perfection at the opposite end?" and upon this you say, might not this query be made with as much reason in the case of motion and rest?—and in any case of the intermixture of opposite qualities, may it not be said, the moment we leave the element of perfection at one end, we involve the element of perfection at the opposite?—may it not be said of light and darkness, or of opaqueness and translucency? and so forth.

33. I admit that these questions are very properly put; not that I go to the full extent of them, as for instance that of motion and rest; but I do not perceive their bearing upon the question, of whether conduction and insulation are different properties, dependent upon two different modes of action of the particles of the substances respectively possessing these actions, or whether they are only differences in *degree* of one and the same mode of action? In this question, however, lies the whole gist of the matter. To explain my views, I will put a case or two. In former times a principle or force of levity was admitted, as well as of gravity, and certain variations in the weights of bodies were supposed to be caused by different combinations of substances possessing these two principles. In later times, the levity principle has been discarded; and though we still have imponderable substances, yet the phenomena causing weight have been accounted for by one force or principle only, that of gravity; the difference in gravitation of different bodies being considered due to dif-

ferences in *degree* of this *one force* resident in them all. Now no one can for a moment suppose that it is the same thing philosophically to assume either the two forces or the one force for the explanation of the phenomena in question.

34. Again, at one time there was a distinction taken between the principle of heat and that of cold: at present that theory is done away with, and the phenomena of heat and cold are referred to the same class, (as I refer those of insulation and conduction to one class), and to the influence of different degrees of the same power. But no one can say that the two theories, namely, that including but one positive principle, and that including two, are alike.

35. Again, there is the theory of one electric fluid and also that of two. One explains by the difference in degree or quantity of one fluid, what the other attributes to a variation in the quantity and relation of two fluids. Both cannot be true. That they have nearly equal hold of our assent, is only a proof of our ignorance: and it is certain whichever is the false theory, is at present holding the minds of its supporters in bondage, and is greatly retarding the progress of science.

36. I think it therefore important, if we can, to ascertain whether insulation and conduction are cases of the same class, just as it is important to know that heat and cold are phenomena of the same kind. As it is of consequence to shew that smoke ascends and a stone descends in obedience to one property of matter, so I think it is of consequence to shew that one body insulates and another conducts only in consequence of a difference in degree of one common property which they both possess; and that in both cases the effects are consistent with my theory of induction.

37. I now come to what may be considered as queries in your letter which I ought to answer. Paragraph 8 contains one. As I concede that particles on opposite sides of a vacuum may perhaps act upon each other, you ask, "wherefore is the received theory of the mode in which the excited surface of a Leyden jar induces in the opposite surface a contrary state, objectionable?" My reasons for thinking the excited surface does not directly induce upon the opposite surface, &c., is, first, my belief that the glass consists of particles conductive in themselves, but insulated as respects each other (17); and next, that in the arrangement given 4, 9, or 10, A does not induce directly on C, but through the intermediate masses or particles of conducting matter.

38. In the next paragraph, the question is rather implied than asked—what do I mean by polarity? I had hoped that the paragraphs 1669. 1670. 1671. 1672. 1679. 1686. 1687.

1688. 1699. 1700. 1701. 1702. 1703. 1704., in the *Researches* would have been sufficient to convey my meaning, and I am inclined to think you had not perhaps seen them when your letter was written. They, and the observations already made (5. 26.), with the case given (4. 5.), will, I think, be sufficient as my answer. The sense of the word *polarity* is so diverse when applied to light, to a crystal, to a magnet, to the voltaic battery, and so different in all these cases to that of the word when applied to the state of conductor under induction (5.), that I thought it safer to use the phrase "species of polarity," than any other, which being more expressive would pledge me further than I wished.

39. Paragraph 11 involves a mistake of my views. I do not consider bodies which are changed by friction, or otherwise, as polarized, or as having their particles polarized (3, 4. 27.). This paragraph and the next do not require, therefore, any further remark, especially after what I have said of polarity above (38.).

40. And now, my dear sir, I think I ought to draw my reply to an end. The paragraphs which remain unanswered refer, I think, only to differences of opinion, or else, not even to differences, but opinions regarding which I have not ventured to judge. These opinions I esteem as of the utmost importance; but that is a reason which makes me the rather desirous to decline entering upon the reconsideration, inasmuch as on many of their connected points I have formed no decided notion, but am constrained by ignorance and the contrast of facts to hold my judgment as yet in suspense. It is, indeed, to me an annoying matter to find how many subjects there are in electrical science, on which, if I were asked for an opinion, I should have to say, I cannot tell,—I do not know; but, on the other hand, it is encouraging to think that these are they which if pursued industriously, experimentally, and thoughtfully, will lead to new discoveries. Such a subject, for instance, occurs in the currents produced by dynamic induction, which you say it will be admitted do not require for their production intervening ponderable atoms. For my own part, I more than half incline to think they do require these intervening particles, that is, where any particles intervene (1729. 1733. 1738.). But on this question, as on many others, I have not yet made up my mind. Allow me, therefore, here to conclude my letter; and believe me to be with the highest esteem,

My dear Sir,

Your obliged and faithful Servant,

M. FARADAY.

Royal Institution, April 18, 1840.

XVI.—*Experimental and Theoretical Researches in Electricity, Magnetism, &c.* By WILLIAM STURGEON, Lecturer on Experimental Philosophy at the Honourable East India Company's Military Academy, Addiscombe.—Superintendent of the Royal Victoria Gallery of Practical Science, Manchester, &c. &c. Fifth Memoir.

Section 1.

On Voltaic Combinations.—A new Battery of Cast Iron and amalgamated Zinc.—A comparison of the Chemical powers of various Voltaic Batteries.

234. About twelve years ago, I engaged in an extensive series of experimental enquiries, respecting some of the principal conditions necessarily connected with the action of voltaic batteries; during which, I arrived at some remarkable results, which I then conceived might probably be advantageously applicable in the formation of that peculiar class of electrical apparatus. Some of these results I published in the year 1830, in a pamphlet entitled “*Experimental Researches in Galvanism, Electro-magnetism, &c.*”^{*} Since the time of my pamphlet making its appearance, some of those results which I described in it have become available in the hands of other experimenters, and some others have come into general use in almost every form of voltaic battery.[†] There are, however, discoveries which I then made and intended for the second part of that pamphlet, and as they have not yet been met with by others, nor in any way made public, only occasionally at my lectures; and as they appear to be of some importance, whether viewed as theoretical or practical data, I venture to give them a place in this memoir.

235. In the pamphlet already alluded to, I have shewn, at page 44, that when two similar pieces of iron are placed, one in each of two strong solutions of nitric acid in water, of different degrees of strength, having a bladder partition between them, they formed an active voltaic pair. A galvanometer with a heavy needle, four inches long, supported on a pivot, was employed in these experiments, “and the needle would frequently stand at an angle of 35° particularly if the stronger portion of the acid solution be not very feeble, and these

^{*} This pamphlet is published by Sherwood, Gilbert, & Piper, Paternoster Row, London.

[†] In the pamphlet alluded to, I pointed out and shewed by conclusive experiments the superiority of rolled zinc over cast zinc, in voltaic arrangements.

energies seem to improve with an increase of acid in that portion of the fluid."

236. At page 45, of the same work, (paragraph 49), under the head "iron and nitrous acid," I have shewn that, "the electric relations of the two pieces of polished iron when placed in two portions of this acid, very differently diluted, or the one piece in the acid solution and the other in water, are precisely of the same character as when the *nitric* is employed; but the electrical energies displayed are more energetic, &c."

237. From the facts discovered in these experiments, I was led to construct a compound battery of ten small pairs of iron plates, in wooden cells; each cell being furnished with a bladder partition. The iron which constituted what I have called "*a pair*," was, however, merely a single piece, or long strip, which, by being bent in the middle, was easily adapted to unite two troughs: one of its ends being immersed in the *strong* acid solution, and the other end in the *feeble* acid solution of the vicinal trough; and so on throughout the series. With this battery I could decompose water, ignite metals, charcoal, &c. to a certain extent as decidedly, as by any voltaic battery whatever; but as its chemical and calorific powers did not meet my expectation, I proceeded no farther with it. I discovered however, that iron held a more elevated rank amongst the metals when associated with amalgamated zinc, in voltaic series, than had ever been noticed by any other experimenter. Indeed, at that time amalgamated zinc had never been employed in voltaic batteries, except in a semi-liquid form by Mr. Kemp, an ingenious chemist at Edinburgh. Sir Humphrey Davy first noticed that amalgamated zinc acted better than pure zinc when associated with copper, in a single pair; but I believe that the employment of amalgamated rolled zinc originated with my own experiments: * and I formed compound batteries of cylinders of zinc and copper which worked exceedingly well with diluted sulphuric acid.

238. I discovered also that cast iron and wrought iron performed very differently in voltaic combinations with zinc, the cast iron forming the more energetic combination with that metal, especially when well amalgamated. I discovered moreover, that amalgamated iron holds a higher rank than either cast iron or wrought iron, when voltaically associated with zinc, and that, therefore, any transference of mercury that might occur from amalgamated zinc would rather be favorable

* Zinc may be easily amalgamated by first immersing it in dilute sulphuric acid and then in mercury. See p. 41, of my pamphlet.

to the action, than otherwise, a circumstance so diametrically opposed to that which occurs with amalgamated copper as to give a preference to iron over that metal in voltaic associations with amalgamated zinc, especially when excitation is carried on with dilute sulphuric acid. Lately I have been induced to construct larger batteries of cast iron and amalgamated zinc, than I had ever before done, which, with their performances in the display of phenomena, I will now describe.

239. The first battery of this kind, that I constructed since my appointment at this Institution, consists of ten cylindric jars of cast iron, each 8 inches high and $3\frac{1}{2}$ inches diameter, with the same number of amalgamated zinc cylinders of the same height as the iron ones, and about 2 inches diameter. Each pair of these metals is connected together by means of a curved stout copper wire, one end of which being soldered to the iron, and the other to the zinc, as shewn in fig. 7, plate 1.* The zinc of one pair is placed in the iron jar of the next, and so on throughout the series: contact being prevented by discs of millboard placed in the bottoms of the iron vessels. Before any regular or exact experiments were carried on with this battery, a few trials were made with it to give an idea of its probable powers; some of which are the following:

240. *Experiment 1.*—When six pairs were arranged in series, and charged with dilute sulphuric acid, the polar wires were properly connected with an electro-gasometer, whose terminal platinum plates are $2\frac{1}{2}$ inches high, and $1\frac{1}{2}$ broad; consequently exposing a surface of upwards of 11 square inches to the acidulated water† in the instrument. The terminals gave off 2 cubic inches of the mixed gases per minute.

241. *Experiment 2.*—By adding two other pairs to the last series, and arranging the whole in a series of 8 pairs, the terminals in the electro-gasometer liberated $7\frac{1}{2}$ cubic inches of the mixed gases per minute. The above results were obtained several times over, and, in some cases, after the battery had been in action for more than three quarters of an hour.

242. *Experiment 3.*—The electro-gasometer was now laid aside, and the calorific effects of the eight pairs in series were as follow:—

Charcoal gave out a small star of brilliant light.

One inch of copper wire $\frac{1}{2}$ of an inch diameter was fused.

Four inches of do. made white hot.

* This figure will also appear in plate 4, which will also contain several other figures illustrative of certain parts of this memoir.

† The liquid in the electro-gasometer was 6 water, and 1 sulphuric acid, by measure.

Eighteen inches of do. made red hot in broad day light.
Eight inches of watch main spring was made red hot.

Two inches of do. made white hot for several successive minutes.*

243. *Experiment 4.*—The battery had now been in action more than an hour, and its decomposing powers were again ascertained to be equal to those exhibited at first, the terminal platinum plates still liberating the mixed gases at the rate of $7\frac{1}{2}$ cubic inches per minute. The voltaic series, on this occasion, was not extended beyond eight pairs, in consequence of the other two iron jars being leaky, and could not be used until the fissures were repaired.

244. *Experiment 5.*—As the exhibition gallery of this institution was shortly to be opened to the public, I was requested by some of our directors to try if this battery could be used to illustrate the explosions made by Colonel Pasley against the wreck of the Royal George. For this purpose, the series of eight pairs was furnished with two conducting wires, 200 feet in length each, making a circuit of 400 feet long. When the farthest extremities of these wires were joined by a thin platinum wire, the latter instantly became red hot, which left no doubt of the calorific powers of the battery being capable of exploding gunpowder at that distance; but as no preparations had been made for trying its calorific effects below the surface of a body of water, nothing farther was done at that time.

245. *Experiment 6.*—On Saturday afternoon, the 30th of May, some of our directors and a few other gentlemen, met in the gallery, and it was proposed to try the iron battery again: and as the two leaky jars (243.) were now repaired, the whole ten were arranged in one voltaic series, and charged, as before, with dilute sulphuric acid. The electro-gasometer which had been used in the former experiments, (240.) having been broken by accident, another, of much larger dimensions was now employed. Its terminal metals consist of two sheets of thin platinum, exposing about 144 square inches of surface to the acidulated water in the apparatus.† When the ten pairs, in series, were properly connected with the terminals of this instrument, 15 cubic inches of the mixed gases were liberated per minute. In the course of about eight minutes' action, the rate of decomposition sank to about 13 cubic inches per

* In the short description of this battery given at page 67 of this volume, I have said that 10 pairs were used to produce these calorific effects, but I find by my notes that only eight pairs were used.

† This electro-gasometer is that which was used with Mr. Grove's battery, at the Royal Institution of Great Britain. See *Annals of Electricity*, vol. 4, p.

minute; and after a quarter of an hour's action, it became reduced to about 11 cubic inches per minute.

246. *Experiment 7*.—Preparations were now made for imitating the 'blowing-up of the Royal George,' but 'as no water could be let into the basin of the canal in the exhibition room of the Institution, in consequence of the painters being at work in it, we had recourse to a very humble, and to some persons it will appear, a most ridiculous substitute; viz., a bucket of water. Our charge of gunpowder was the same as that used in the Polytechnic Institution in London, being furnished with a stock of cartridges, from Messrs. Watkins and Hill, Charing Cross, which had been made for similar illustrations in that Institution. The bucket of water being placed on the floor of the lecture room, and one of the extremities of each long conducting wire (244) being twisted to the wires of the cartridge, the other extremity of one of them was attached to one pole of the battery, situated in the passage outside of the room door. When the word *fire* was given, and the circuit completed by Mr. Brookhouse, who stood by the battery, with the other connecting wire, for that purpose, the most singular phenomenon occurred that was ever beheld by any of the party present; and certainly one which none of us had been led to expect. The explosion of the gunpowder was accompanied by a simultaneous perpendicular ascent of both bucket and water into the air, where they seemed to rest, for a moment, at an altitude of about $5\frac{1}{2}$ feet above the floor, when both fell, and the greater part of the water spilled on the floor. The singularity of this anticlock of the bucket produced an effect on the bystanders more easy to imagine than describe: every one involuntarily burst into an immoderate fit of laughter, which became more and more excited as each person described the ludicrousness of the event; and the consternation displayed by the two servants, who were present, in finding mops, basins, and other paraphernalia, with which they were not prepared, for taking up the water from the room floor, added no little to the burlesque character of the scene. However, the two men were very active, and in a short time the most of the water was in the bucket again.

247. *Experiment 8*.—When the effect of the last *blow-up* had sufficiently abated, one of our directors proposed that the experiment should be repeated, in order to ascertain how high the bucket and water could be raised by a second explosion. The necessary preparations being made, and chairs, forms, tables, &c., being removed from the vicinity of the bucket; the glass cupboard, in which our splendid electrical machine is placed, being guarded by chairs, forms, &c., against the

effects of splinters in case of the bucket giving way to the force of the powder, and the faces of glazed pictures turned to the wall, &c., the cartridge was sunk in the water; and on the word *fire* being given the explosion again took place. The bucket jumped up to the height of about $4\frac{1}{2}$ feet from the floor on to the lecture table, carrying with it only a small portion of water, the rest being scattered about in every direction. The servants, who were prepared, on this occasion, to take up the water from the floor, set to work with great alacrity in hopes to be enabled to replace the greater part of it in the bucket in a few minutes; but observing, after working a short time, that with all their efforts they were not lessening the water on the floor, one of them looked to see how much had been collected in the bucket, and immediately called out, that "the bottom was blown out!" Nothing better than this news could possibly have happened, to give increased tension to the already excited risibility of the company.

248. The cause of the bucket and its water jumping up together by the first explosion, may probably be traced to the sudden reaction of the floor against the bottom of the bucket: which rebounded with a force nearly equal to that with which the water was blown upwards, and being in the same direction they kept pace with one another.

249. *Experiment 9.*—The battery had now been charged more than an hour, and its decomposing powers were again tried with the same electro-gasometer as last used. From a mean of several trials the liberated gases amounted to more than 10 cubic inches per minute.

250. Since the appearance of my pamphlet in 1830, experimenters have turned their attention to the improvement of voltaic batteries, and several kinds have been invented, each of which has its peculiarities, and, for some processes, most of them have a great advantage over those previously in common use. It seems rather doubtful, however, from the facts hitherto in our possession, that we shall ever discover a form of battery capable of exhibiting every class of electric phenomena to the best advantage. It is true that with the command of an extensive series of movable combinations or pairs, we can arrange them in groups, or in series in a great variety of ways, and thus be enabled to modify their forces so as to become advantageously available for the display of the electro-magnetic, electro-chemical, and the electro-calorific classes of phenomena; but for the display of the purely electrical phenomena, such as the attractions and repulsions, and the charging of coated glass, the original pile of Volta still stands pre-eminent; and amongst all the forms of *battery* which

have hitherto made their appearance, that of Cruickshank's is the only one which can be advantageously employed for purposes of this kind, and for medical treatment it seems better adapted than any other.

251. The batteries severally invented by Grove, and Smee, are unquestionably about the most powerful now generally known for continued action in the electro-magnetic, electro-chemical, and electro-calorific departments; but their high price almost precludes their general employment amongst experimenters, excepting in such cases as where the funds of an institution are at command. Professor Daniell's battery is also so constructed as to retain its powers in action for a long time together, but unless of large dimensions, its chemical, magnetic, and calorific powers, are far below those of the former two batteries. Besides the first cost of Grove's and Daniell's batteries, there is a continual current expense attending their preparation and keeping in order for experiment, to which Smee's battery is not subject: for diluted sulphuric acid being the only liquid used, and having no diaphragms between the metals, the excitation is accomplished at a cheap rate, and is not complicated by appendages which are expensive in every form they have hitherto assumed, not only in the first purchase of the battery, but by the frequent renewal of those which become destroyed, and the time necessarily required for their preparation.

252. Notwithstanding the advantages obtained by the great superiority in the action of the modern forms of battery over that exhibited by those invented respectively by Cruickshank and Wollaston, but very little seems to have been done towards ascertaining their real capabilities, as to the most advantageous display of the several classes of phenomena to which they are best adapted: hence it is, that their full powers are but little, if at all known. It is thus that an important inquiry is still left untouched, which may probably reveal facts of the highest interest to this department of physical science. Moreover, as the employment of voltaic batteries has now become very extensive, not only in investigations, but in the daily illustrations at this, and many other similar institutions, and is likely to be still more extensively employed, both in military and civil engineering, it is obvious that a cheap efficient battery, with the mode of conducting it to the best advantage, are desiderata of great moment to the practical man who may have occasion to avail himself of the advantages which such an implement affords in the daily processes of his professional avocations. But an investigation such as is best adapted to reveal these important facts, would require the command of

every kind of battery that appears likely to be adapted for general purposes, to which such an implement³ is peculiarly applicable: and although not much skill in manipulation would be absolutely essential to such an undertaking, the requisite series of experiments would be somewhat expensive, and could not be conducted without a considerable occupation of time.

253. The batteries belonging to this institution are the following, viz.:—Cruikshank's, two troughs of 50 pairs of 3 inch plates each.—Wollaston's, two troughs of 10 pairs of 4 inch plates, with double coppers each.—Daniell's, 20 copper cylindrical jars, 24 inches high and 4 inches diameter, with amalgamated strips of rolled zinc, in hempen bags or diaphragms. Grove's, 50 pairs of 4 inch platinum plates, with double amalgamated zinc in porous pots for diaphragms. Besides these, we have 30 of those cast iron jars, with their amalgamated zinc cylinders already described, (239), and 20 pairs of copper and amalgamated zinc cylinders, in porcelain jars. I have availed myself of the use of these batteries, and also of one of Smee's construction of twelve pairs, which, by the kindness of Mr. Joseph Lockett, has been placed in my hands for the purpose of comparing their powers in the display of the electro-chemical, electro-magnetic, and the electro-caloric classes of phenomena, and for ascertaining which kind of battery is most likely to become more generally useful, both as regards economy and facility of manipulation.

On the Chemical Powers of Voltaic Batteries.

254. The chemical powers of our modern batteries have, hitherto, been tested in no other way than by the decomposition of acidulated water. This circumstance may probably be owing to the great facilities which are afforded by operating on this compound, and the *supposed exactness* of the results. In point of preparation and manipulation there can be no doubt of the superior facilities for the decomposition of water, over that of most other bodies; but notwithstanding the facilities thus afforded to experimenters, the decomposition of water, as a test for the powers of voltaic batteries, has led many to the most extravagant inaccuracies: and I am not aware that any experiments are on record that have been directed to an enquiry for ascertaining the best means of arriving at a maximum of decomposition by the employment of any one of the several batteries which have hitherto been constructed. The errors of a fashionable man, whatever may be the nature of his pursuits, are almost sure to lead those astray who have either no desire or no opportunity to judge for themselves,

and there is not, perhaps, amongst the numerous errors into which Dr. Faraday has fallen, one more eminently calculated to mislead the unwary experimenter, than the pretended accuracy of the indications of an instrument, the principles of which, he either neglected to reveal, or of which he had not the slightest knowledge. The *visionary voltameter* has been a favorite instrument with experimenters, only because of their credence in the assertions of its author, and some of them have thus been led into errors which would otherwise have been avoided, amongst the records of their own discoveries.

255. If we wish to arrive at a knowledge of the powers of any voltaic battery in the process of decomposing water, there are several particulars which are necessary to be attended to: some of which will vary with almost every form of battery, whilst others are common to all batteries whatever.

256. The first essential point to be determined is, which is the most influential body in facilitating decomposition when dissolved in the water to be operated on? And as that solution which facilitates decomposition the most in one case, will also facilitate it to the greatest extent in all, whatever may be the form of battery employed, the determination of this point becomes easily accomplished. A solution of sulphuric acid is now generally placed in connexion with the platinum terminals in the decomposing apparatus: and I have not found any other which facilitates decomposition to the same extent, when the water is to the acid as about 5 to 1. The mixture ought to be made some hours prior to its being placed in the apparatus, otherwise its heat will soften the cement so as to give way to the liquid pressure, and become leaky. Whatever may be the real character of the action of bodies which facilitate the decomposition of water:—whether it be a mere mechanical separation of its particles, which makes them more assailable to the electric forces;—an improvement in its electro-conduction, and thus permits the introduction and consequent flow of a greater quantity of electric fluid; or whether it admits of an improved electro-polarization by an association with the particles of the dissolved body, remains a problem, for which philosophers have not yet found a solution.

257. The second consideration is the *distance* between the platinum terminals in the decomposing apparatus, which can hardly be too small, provided they do not absolutely touch one another. This is a fact generally known, and like the former particular, applies to all batteries whatever.

258. The third thing to be determined in the decomposition of water, is the *size* of the terminal metals in the decomposing apparatus: for the extent of decomposition will vary very

considerably with terminals of different extent of surfaces. With feeble batteries, it is necessary to concentrate the electric force to a mere point before any decomposition of water can be accomplished; hence, in such cases, short thin platinum wires are preferable to terminals of larger dimensions. The decomposition of water, however, is not the best test for ascertaining this law with precision, when the intensity of the battery is very feeble. Perhaps the following experiment will answer as well as any.

259. *Experiment*.—Employ a battery of one pair only, of small dimensions, and let the liquid operated on be a strong solution of sulphate of copper. Let the terminal metals be sheets of platinum foil of 3 or 4 square inches each; and immerse them both completely in the cuperous solution. No decomposition is perceptible, even though the connexions be continued for more than an hour: but a galvanometer placed in the circuit, indicates the existence of a current. Let, now, the negative terminal be taken out and wiped dry, and then immerse only one of its corners. In a few minutes the immersed corner will be covered with precipitated copper, indicating decomposition by the force of the concentrated current at that point: but the galvanometer needle indicates a much feebler general current than when the platinum plate was wholly immersed. By immersing the corner of the platinum terminal to different depths in the solution, the exact amount of metallic surface which just allows of decomposition, may be discovered. And it will be found, in all cases, that as the immersed surface increases, the magnetic deflections increase also. Hence it becomes obvious that the powers which such feeble currents exercise on a magnetic needle are no indications of the chemical powers of the battery; unless, indeed, we look for the one as the reverse of the other. There are several interesting facts on this nice subject; but as the principal object of this memoir is to investigate the powers of the most formidable batteries known, I shall not dwell upon them till a future opportunity presents itself.

260. The fourth point to be determined to effect the maximum of decomposition of water, by voltaic electricity, is *the proper extent of the voltaic series*, or of the proper unit of *intensity* of the battery: and as the intensities of different batteries with the same extent of series, differ very much from each other, the determination of this point must be of great interest to experimenters generally.

261. Having now pointed out four grand particulars to be attended to for obtaining a maximum decomposition of water by voltaic electricity, I will next proceed to describe the

results of a few series of experiments made with the various kinds of batteries already noticed.

Table of Experiments on the Decomposition of Water, with various Series of Professor Daniell's Voltaic Battery; with the two Electro-gasometers described in (253).

No. of pairs in series.	Quantity of Gas obtained per Minute.	
	From the Large Terminals. ()	From the Small Terminals. ()
10	9½	9
9	9¼	8½
8	7½	7½
7	7—	6½
6	5½	5½
5	5½	5
4	3½	3½
3	2	1½
2	Scarcely any from either.	

262. Each of the above tabulated results, is the mean of several trials; they furnish us with a knowledge of the *unit of intensity*, of this kind of battery, which is obviously that given by a series of 5 pairs. And although the decomposition by an extensive battery, would not suffer much loss by employing a series of either 6 or 7 pairs, yet any series above 7 or below 5, would be attended with a great loss in the *quantity* of decomposition in a given time.

263. Another essential feature in these results, is in the quantities of gas liberated by the different sized terminals; the larger ones invariably producing the greater quantity.

264. In another series of experiments with Mr. Daniell's battery, and the electro-gasometer with the larger plates (245), I obtained 10½ cubic inches of the mixed gases per minute, with a series of 10 pairs; and with lower series, the rate of decomposition was nearly proportional to that in the above table; thus indicating by both sets of experiments, that the proper unit of intensity is a series of 5 pairs: for by employing the ten pairs in two series of 5 pairs each, I obtained above 12 cubic inches of the gases per minute.

265. *Table of Experiments on the Decomposition of Water, by various Series of Voltaic Pairs of Cast Iron and amalgamated Zinc, as described in paragraph (239).*

No. of Iron Jars in series.	Cubic Inches obtained per Minute.	
	Large Terminals.	Small Terminals.
10	14	10
9	11	8½
8	10	7
7	7½	5
6	4	2½
5	2½	2
4	1	½
3	Scarcely any.	

266. The first thing to be observed in this table, is the superiority of action by the large terminals, over that by the smaller ones; and in a much greater degree, than by Daniell's form of battery.

267. The next thing to be observed is the rapid increase of decomposition, by an increase of the voltaic series, even up to ten pairs; by which we understand that the whole in one series, is much more powerful than in any other way we could combine them; and it is probable, that by extending the series we should discover that the proper unit of intensity, is considerably greater than that given by ten pairs.

268. The above results were by the employment of the first ten pairs, of this kind, that were constructed; but since the time the above experiments were made, I have obtained 22 cubic inches of the mixed gases per minute with the 10 pairs in series; I have also got 20 new iron jars cast; with 10 pairs of which I have obtained 99 cubic inches of the gases in four minutes action: and I am in hopes of arriving at a still greater rate of decomposition. In all cases with the iron batteries, the decomposition has increased rapidly up to ten pairs in series, indicating that a still higher intensity is required for the most advantageous *unit of intensity*.

269. *Table of Experiments on the Decomposition of Water, by various Series of Voltaic Pairs, on the principle of Mr. Smee's Battery. The Electro-gasometer, with large Terminals, (245) was the only one employed in this series of experiments.*

No. of Pairs in Series.	Cubic Inches of Gases liberated in One Minute with large Terminals. ()
2 Scarcely perceptible
3 Ditto
4 $1\frac{1}{4}$
5 1
6 3
7 8
8 11
9 13
10 15

270. If we look to the rapid increase of decomposition from a series of 6 pairs to the series of 10 pairs, we are soon convinced that to employ a series of 10 is more advantageous than any series below that number; and it is very probable that the proper *unit of intensity* with this battery, as with the cast iron one, is considerably above that given by a series of 10 pairs. This point, however, must be determined by future experiments, as I have not, at present, more than 10 pairs at command. But the experiments detailed in the above table,

will be a sufficient guide, for the present, for any person employing no more than 10 pairs at once, because it is obvious that the decomposition of water will be accomplished to the greatest extent, by employing them in one series : which also appears to be the case with the cast iron battery.

271. *Experiments on the Decomposition of Water, by various series of Voltaic Pairs, upon the principle of Mr. Grove's Battery. The decomposing apparatus with the larger terminals was used 245.*

No of Pairs in Series.	Cubic Inches of Gas per Minute.
2Scarcely any.
36
49
511
614
716
818
921
1024

272. From the results of this series of Experiments, it is obvious that the 10 pairs in series produce more decomposition than by any other combination of them ; and it is probable that a still more extensive series would be the proper *unit of intensity* for accomplishing the maximum of decomposition by this kind of battery. Mr. Grove has, I believe, constantly employed his battery in series of 5 pairs only, which series is obviously too small, and occasions a considerable loss of decomposing power.

273. Suppose, for instance, that a battery of 30 pairs were to be used, in six series of 5 pairs each, then as 5 pairs give 11 cubic inches of gas, $5 \times 6 = 30$ pairs, would give $6 \times 11 = 66$ cubic inches. But 30 pairs in three series of 10 pairs each would give $3 \times 24 = 72$ cubic inches of gas, which is six cubic inches more than by Mr. Grove's mode of combination.

274. In order to compare the decomposing powers of these batteries, it will be necessary to ascertain their *relative* metallic surfaces exposed to the exciting media. They stand as below for each pair :—

Daniell's	=360 square inches of metallic surface.
Smee's	...=192 do.
Sturgeon's	=162 do.
Grove's	...=104 do.

275. Thus, by assuming Mr. Grove's battery as the unit of

surface, and also the standard of decomposing power, we shall have :

Metal.	Gas.	Metal.	Gas.	
104	24	Grove's	
162	: 25 : :	104	: 14.8	Sturgeon's
192	: 15 : :	104	: 8.1	Smee's
360	: 12 : :	104	: 3.5	Daniell's

276. Hence it appears, that if the whole of the batteries exposed precisely the same extent of metallic surface to the existing liquid, that invented by Mr. Grove would have a decided preference, and Professor Daniell's battery would hold but a very low rank in point of decomposing power. But if we view them individually according to their respective sizes in which they have been employed in these experiments, then their maximum powers that I have obtained, will stand thus :

Sturgeon's ...	25	Cubic inches of gas per minute.
Grove's	24	do.
Smee's	15	do.
Daniell's.....	12	do.

277. The next consideration is the cost of these batteries, both as relating to the first purchase, and the current expense of keeping them in action. The price given for 12 pairs of Smee's construction, Mr. Lockett informs me, was £32. Hence the price of 10 pairs would be £26 13s.*—The price of 10 pairs of each of the other kind of batteries is, Grove's £7.—Daniell's £6.—Sturgeon's £3 10s.

278. The excitation is carried on by about the same quantity of sulphuric acid in each battery ; and in Smee's, and the iron batteries, no other expense is required. But in Grove's battery $1\frac{1}{2}$ lbs. of the best nitric acid for 10 pairs is used in addition : and in Daniell's, about 5 lbs. of sulphate of copper, in addition to the sulphuric acid, is used for 10 pairs. In both these latter batteries, there are also diaphragms which are continually falling into decay, which is another current expense attending these batteries. The mercury employed in the amalgamation of the zinc, would be nearly the same in all the forms of battery hitherto described ; but the time occupied in fitting up is very different indeed : the iron battery requiring much less time than any of the other forms. Hence as far as the decomposition of water is concerned, the iron battery has a decided advantage, both in point of power and

* There can be no question, of this being a very extravagant price, as I am confident that it can be had for less than half that money, either from Watkins and Hill, Clarke, Carey, Jones, Newman, or Harris.

economy : and is so simple, that it is manageable by any person : and what is another point in its favor, it works best when quite rusty : and retains its power a long time. The hydrogen is certainly an annoyance, but I have hit upon a contrivance to remove it, which I shall describe in the sequel.

(*To be continued in the September Number.*)

XVII.—*Description of a New Compensating Pendulum.*

By WILLIAM GWYNN JONES, A. M.

(*Extracted from Silliman's Journal.*)

During the latter part of the past year, while engaged in some interesting astronomical observations which required considerable accuracy, it was indispensable to procure a time-keeper whose rate would not be affected by the variations in the temperature of the weather, to which all such machines, of ordinary construction, are liable. The expensiveness of a chronometer which could be relied upon for such a purpose, rendered a resort to some more economical instrument desirable, if it could be depended upon. The gridiron pendulum as well as the mercurial one, both of which have been designed to effect this object, were found unsatisfactory ; the former from the difficulty of procuring an exact adjustment of the different rods of which it is composed, so as to produce the desired counterbalancing expansion and contraction, and the mercurial pendulum proving upon experiment too sensitive to be relied upon. Under these circumstances, I contrived a simple arrangement for a pendulum, acting upon the principle of the lever, which performed with so much accuracy that I have been induced to present it to the notice of the readers of the *American Journal*, believing it will not prove uninteresting to those engaged in scientific investigations requiring great uniformity of action in a time-keeper. The arrangement of the parts is so simple as to be readily understood by any skilful workman, and as it is entirely free for the adoption of any one who may prefer its construction, I have prepared a description and diagram to render it intelligible.

Fig. 5 plate 3, shews the whole pendulum, the dotted lines representing similar parts to those on the opposite side, and are introduced to render the drawing more easily understood ; *a* is a similar spring to that which is attached to the pendulum of an ordinary eight-day clock, and is firmly attached to the perpendicular brass bar *b*. Through *b* there is the usual opening for the guy-wire, which gives motion to the pendulum,

This bar is firmly affixed to the transverse bar *c* either by riveting or soldering. On each end of the bar *c* there is attached a brass rod *d*, *d*, and one inch from each of these there is also affixed a steel rod *e*, *e*. These four rods pass through the bar *p*, which is intended merely to preserve them in their proper position, and is attached to the two brass rods by a pin passing through both, while the steel rods are allowed to move freely through the holes. At *f*, a transverse bar or lever is affixed to *d* by a loose pin passing through them, and the same attachment is made to the steel rod *e* at *g*. This bar is four inches long, three inches of which extend from *g* to *h*, and a similar one is attached to the dotted rod *d* and extends on the opposite side. At *h* there is another loose attachment to the rod *i*, which is of steel, and which is again affixed to the bar *k*. At *k* there is a permanent bar *m*, which passes through the weight *o*, and has the usual adjusting screw *n* at the bottom.

Rationale.—Suppose that by an increased temperature of 20° , the steel rods *e*, *e*, are expanded in length $\frac{1}{16}$ of an inch. The rods *d*, *d*, being of brass, and a small fraction larger than the steel, will expand $\frac{1}{8}$ of an inch by the same increase of temperature, it being an established theory with the best French chemists, that the relative effect of the temperature upon the two metals is as 3 to 5, or nearly double the expansion in brass as in a steel rod of similar size. The outer rods then have expanded in length $\frac{1}{8}$ of an inch more than the inner rods. It will be apparent from a slight inspection of the drawing, that as the brass rod *d* and the steel one *e* are attached by a connecting pin to the transverse bar *fh*, that by *d* expanding more than *e*, that *fh* becomes a lever, *g* being the fulcrum, and as *gh* is three times as long as *fg*, consequently if *d* be expanded $\frac{1}{16}$ more than *e*, the end *h* will be elevated $\frac{3}{16}$ of an inch, and thereby raise the weight *o* $\frac{1}{8}$ of an inch more than the expansion of *d* has depressed it. This increased elevation is intended to allow that the spring *n* the bar *b*, the rod *i*, and the bar *m*, unitedly, will expand $\frac{1}{8}$ of an inch also, and if so, it must be apparent that the whole pendulum has preserved its equilibrium and remains precisely of the same length as if no change had taken place in any of its parts.

Fig. 6 plate 3, shews a perpendicular view of the transverse bar *fh*, arranged so as to admit the corresponding bar for the other side to work freely, and at the same time preserve the four upper rods upon a line with each other, which, as the levers intrude within each other, could not be done without the recess as shewn in the section. The same letters correspond

to the same parts in Figs. 5 and 6. The dotted lines in Fig. 6, are intended to shew the relative position of the lever which is attached to the dotted line *d*, Fig. 5, in regard to the other.

Baltimore, Md., 1834.

XVIII.—*Description of an Economical Apparatus for Solidifying Carbonic Acid, recently constructed at the Wesleyan University, Middletown, Conn.* By JOHN JOHNSTON, A. M., Professor of Natural Science.

The solidification of carbonic acid has of late excited considerable interest both in Europe and in this country; but the cost of the necessary apparatus has been considerable, and many probably have on this account, merely, been prevented from making any attempt to repeat the experiment. Most of our public literary institutions, in which alone in this country such apparatus is ever used, are obliged to study economy, and they are therefore often liable to be prevented from availing themselves of the benefits of new discoveries like the present, merely on account of the expense of apparatus.

It is therefore thought a description of an economical apparatus for solidifying carbonic acid may be acceptable to the public, though we do not pretend to offer anything new on the general subject.

The generator A, fig. 7 plate 3 is made of a common mercury flask, several of which I have tested and find sufficiently strong. They may be purchased in New York for a dollar a piece, or even less. The aperture at the neck may be a little enlarged, so as to make it an inch or an inch and a quarter in diameter, and the thread of the screw re-cut. A plug of cast-steel B is made of a bar two inches in diameter, and turned with a wide and smooth shoulder, so as to fit accurately upon a collar of block-tin when screwed into its place, as represented in the figure. This collar should be soldered to the iron; which is easily accomplished by filing the iron bright and tinning it in the ordinary manner, and then melting the block-tin and pouring it on, having first screwed a cork into the aperture and formed a wall of putty or clay at a sufficient distance around it. The shoulder of the plug is readily made to fit the collar accurately by screwing it a few times into its place, and then removing with a coarse file the parts of the collar upon which it touches. In this manner an accurate joint may be made without the use of a lathe; and if the plug does not correspond precisely with the axis of the flask it is just as well.

The faucets or stop-cocks are the most difficult part to

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construct, and occasion full half the expense. These in our apparatus are supposed to be essentially the same as are used by others for this purpose, but it may not be amiss to insert a description, since none has to my knowledge been given. There is this peculiarity about ours, however; they are inserted in the cast-steel plugs, which indeed make a part of them. D fig. 8 plate 3 is designed to represent the plug removed from the generator; at the upper end of it a hole F one inch in diameter is drilled about an inch deep, terminating in a hollow cone into which the point G fig. 9, is accurately ground. A small hole extends quite through the plug. Around the aperture F a collar of block-tin is fitted to receive the shoulder of the part E, as seen at I, and prevents any passage around the threads of the screw. Through the axis of the part E a hole three eighths of an inch in diameter is drilled, and receives the part G which is screwed in from below, the handle H being removed. The handle H should be afterwards riveted on.

Now suppose H E G to be inserted in its place in the cast-steel plug, as represented at B I, fig. 7, the plug itself being screwed into the generator. If H' be screwed down, the aperture from the generator is firmly closed by the conical point G; and by giving H' a single revolution in the opposite direction, the shoulder of G is brought firmly against the bottom of E, so that no escape is permitted directly upward, but only in a lateral direction through the brass tube L, which connects the generator with the receiver C. A washer of sheet lead should be placed around the shoulder of G, in order to secure a perfect metallic contact between it and the bottom of E.

The receiver C is made of the best boiler iron, which was strongly welded around a cylinder and a bottom also welded in. It is of the same height as the generator, which is about one foot, but only about two inches in diameter internally, and has a capacity of about one pint. This form enables it to resist much greater pressure than if it was of a larger diameter; and it is rather an advantage than otherwise to have it of the same length as the generator.

A cast-steel plug with stop-cock precisely similar to the one described, screws into the receiver, as the other does into the generator. The tube L screws into the plug which is inserted in the receiver, and the other end, turned to a conical point, fits accurately into a cavity in the plug B, and is held in its place by means of the stirrup screw M. Another stirrup screw N, and block of wood O, secures the receiver C in its place.

To use this apparatus the generator and receiver are sepa-

rated, and the plug B being removed, two pounds of bicarbonate of soda, made into a paste with the same weight of water, are introduced into A, and twenty ounces* of strong sulphuric acid are poured into several lead vessels, made by soldering bottoms in pieces of lead tube a little shorter than the length internally of the generator, and of such a diameter that they will just pass the aperture. These being nearly filled with acid are dropped into the generator, which, after the plug B is inserted, is allowed to lie on one side for fifteen or twenty minutes, or a less time if it is several times rolled over to mix the acid with the soda. The receiver is then attached to it as seen in the figure, by means of the stirrup screws M and N; and if kept sufficiently cool by means of ice, the liquid carbonic acid formed in A will shortly be distilled over into C, the passage between them being of course previously opened by means of the stop-cocks before described.

The stop-cocks are now to be closed and the receiver, which now contains the liquid carbonic acid, separated from the generator. A small tin cup is then to be attached to the tube L, precisely as in Dr. Mitchell's apparatus,† to receive the jet of the acid from the receiver. It is essential that the *liquid* acid should escape into this cup, which is effected by having a small tube pass from the steel plug nearly to the bottom of the receiver, or by inverting the receiver before opening the stop-cock.

The best method of testing the strength of the apparatus, is by means of a hydraulic press, but it can be done as effectually by permitting it to lie, when charged, exposed to the direct rays of the sun, and excluded from currents of air, till the temperature rises to 100° or 110° F. This should be done two or three times before running any risks by venturing to handle the apparatus while charged.

It has been our object to construct an apparatus for forming the solid acid merely, but the gauges for ascertaining the pressure, &c. might of course be added as in Dr. Mitchell's apparatus.

The above apparatus, including the expense of testing three times, cost us about nineteen dollars.

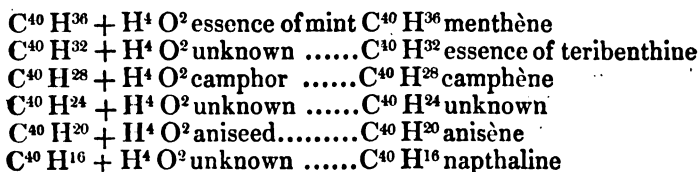
* The quantity of acid required to saturate or neutralize the soda would be a little more than 24 oz., or 22 oz. only if the soda is in crystals, but something less than this should always be used.

† Journal of the Franklin Institute, Vol. xxii. p. 289, and Vol. xxxv. p. 346 of Silliman's Journal.

XIX.—*Organic Chemistry. Memoir on the Essence of Crystallized Peppermint.* By M. WALTER.

(Extracted from the *Comptes Rendus*.)

In a note I had the honor of communicating to the academy relative to the essence of crystallized mint. I endeavored to discover if it were necessary to place this among a group of peculiar bodies, of which, ordinary camphor would be the type; or if its place ought to be in that very nearly related, and at present so numerous, group of alcohols, of which ordinary alcohol is the type. The experiments I have tried decide in favor of the first opinion: in fact, the reactions which are exercised on the essence common sulphuric acid and perchlorure of phosphorus, neat and decisive reactions, of which I shall treat in detail hereafter, are adverse to the idea of considering it as a common alcohol. The group with its derivatives is more numerous than we should at first be tempted to suppose. I have tried to represent it in the following table, in which several bodies are even yet, only hypothetical, and present gaps which I hope ere long will be filled up.



The essence of mint presents itself under the form of colorless prisms, of a taste and smell which belong to the essence of powdered mint. It is rather soluble in water, very much so in alcohol, spirit of wood, ether, and essence of térébenthine; its point of fusion is at 34°c., the point of ebullition 213°c., under the pressure of 0^m.76. Anhydrous phosphoric and ordinary sulphuric acids, perchlorate of phosphorus, dry chloridè acting sometimes in the dark and sometimes assisted by the solar rays, exercise particular reactions. My analyses agree with those of M. Dumas, and the density of the vapor which I have found for him. The following are the data of one of these analyses: 0.3225 essence of mint, 0.9055 carbonic acid, 0.372 water, which gives in centièmes 77.68 carbon, 12.83 hydrogen, 9.19 oxygen: these results agree with the rational formula $\text{C}^{40}\text{H}^{40}\text{O}^2$, which gives 77.27 carbon, 12.62 hydrogen, 16.11 oxygen. The density of the vapor was found 4.62; calculation gives it 5.455. An equivalent of essence contains four volumes of vapor.

Menthène.—Causing anhydrous phosphoric acid to react on the essence of mint, we obtain a particular liquid body to which I have given the name of menthène. Distilling it once or twice over anhydrous phosphoric acid is sufficient to purify it. This liquid is clear, transparent, and of an agreeable smell, its taste is cool; it is soluble in alcohol, ether, &c.; burns with a sooty flame, boils at 163°C ., under a pressure of 0.76; its specific gravity is 0.851 at 21°C . Chlore and nitric acid react in a peculiar manner: brome produces it in a very characteristic deep red colorisation: subjected to analyses it has afforded me the following result: 0.372 menthène, 1.178 carbonic acid, 0.426 water, or in centièmes 87.59 carbon, 12.71 hydrogen. This result agrees perfectly with the formula $\text{C}^{40}\text{H}^{26}$, which would give

$$\text{C}^{40} = 1530 = 87.18$$

$$\text{H}^{26} = 225 = 12.12$$

I took the density of the vapor twice, and found it $=4.9$; the calculation, according to the formula quoted above, gives 4.8. Hence an equivalent of menthène contains 4 volumes of vapor.

Common sulphuric acid when cold exercises no sensible action on the essence of mint: the mixture only takes a red color; but if we heat it in a sea-bath it divides itself into two strata, one colorless and fluid, the other thick and deeply colored with red; the upper stratum supplied several times with cold sulphuric acid exhibits all the characters and composition of pure menthène, the other, thick, saturated with different bases, gave me nothing from which I could infer the existence of sulpho-menthic acid.

Chloro-menthène.—In order to prepare a chlorhydrate of menthène analogous to the chlorhydrates of bicarbonated hydrogen or méthylène, I caused some perchlorure of phosphorus to react on essence of mint; the reaction was very lively, it disengaged abundant vapors of chlorhydric acid. By distilling the whole in a small excess of perchlorure of phosphorus, there passed in the recipient, first, protochlorure of phosphorus, then perchlorure, and finally, an oleaginous body. The mixture supplied with water, caused to appear on the surface of this latter an oleaginous body, which, washed with water and a solution of carbonate of soda, afterwards redistilled twice in perchlorure of phosphorus, washed, put in contact with chlorure of calcium melted, and placed in vacuo, was subjected to analysis.

0.24 of matter gave 0.608 carbonic acid and 0.214 water,

0.3565 of matter decomposed by incandiscent lime, furnished 0³⁴.0 of chloride of silver.

These reduced to centièmes, give

Carbon70.09

Hydrogen..... 9.89

Chlore20.87

They agree with the formula of chloro-menthène, which is

$C^{40} = 69.91$

$H^{34} = 9.77$

$Cl^2 = 20.32$

Chloro-menthène is a pale yellow liquid, its smell is aromatic, resembling that of mace flowers, the taste fresh; it boils at 204°c., and burns with a fuliginous flame edged with green: a concentrated solution of caustic potassa has no effect upon it. Hence collecting these characteristics we may conclude that menthène and chloro-menthène are two bodies of the same type, having the same relationship between them as olefying and chloro-lefying gas, or further, as acetic and chloro-acetic acid.

The action which chlore exercises on the essence of menthène gives rise to compounds of a complicated composition. Causing dry chlore to pass through essence of mint, abundant vapors of chlorohydric acid are liberated, and we at length obtain a yellow liquid more dense than water, which, purified and dried by the ordinary methods and subjected to analysis, gave the following result: 0.338 matter, 0.7 carbonic acid, 0.22 water.—0³⁶⁵ matter gave 0.557 chlorure of silver, or in centièmes,

Carbon49.92	This composition agrees very nearly with the following formula:	$C^{40} = 1530 = 50.4$
Hydrogen ... 6.29		$H^{31} = 193 = 6.3$
Chlore 37.6		$Cl^5 = 1106 = 36.5$
Oxygen		$O^3 = 200 = 6.8$

This product exposed to the action of chlore and solar light becomes more pale, viscous, loses also 6 equivalents of hydrogen which are replaced by 6 of chlore; in short, 0.321 matter employed gave 0.411 carbonic acid, 0.112 water; 0.283 matter furnished 0.643 chlorure of silver. These data reduced to centièmes become

Carbon34.42	Which agrees with the formula:	$C^{40} = 1530 = 35.4$
Hydrogen ... 3.87		$H^{30} = 156 = 3.6$
Chlore56.0		$Cl^{11} = 2434 = 56.3$
Oxygen		$O^3 = 200 = 4.6$

I now pass on to the reactions produced by nitric acid and chlore on menthène.

Cold nitric acid exercises no action; but on warming it, the reaction is made with extreme violence: numerous rectilant vapors and carbonic acid are liberated. At the end the reaction is made with extreme difficulty. We obtain a yellow liquid soluble in water and alcohol, which, purified and submitted to analysis, gave the following result: 0.374 matter, 0.582 carbonic acid; 0.222 water or in centièmes, 43.05 carbon; 6.5 hydrogen, 56.45 oxygen, which nearly agrees with the formula $C^{20} H^{18} O^9$. This acid demands a particular study.

Causing dry chlore to pass through menthène, the chlore attacks it in a very energetic manner, and changes it into a juicy liquid of a yellow color, which, purified and dried in vacuo, gave the following result: 0.311 matter, employed 0.441 carbonic acid, 0.136 water; 0.282 matter, employed 0.653 chlorure of silver, or in centièmes:

Carbon	39. 2	} Which tends to the formula :	$C^{40} = 1530 = 39.18$
Hydrogen ...	4. 8		$H^{26} = 162 = 4.17$
Chlore	5.71		$Cl^{10} = 2213 = 56.67$

In this reaction, the menthène has lost 10 equivalents of hydrogen which have been replaced by 10 of chlore.

All my attempts to produce with essence of mint and the different reactivities of the compounds analogous to those which afford us alcohol, spirit of wood, ether, placed under the same circumstances having failed, the action of sulphuric acid, perchlorure of phosphorus, and phosphoric acid having always given me very particular and novel results, we may conclude that essence of crystalised mint cannot be regarded as an ordinary alcohol. Hence I shall be led to place it in the same group with camphor and acetone, which it very much resembles.

XX.—*Researches on the Phenomena resulting from the introduction of certain Salts in the way of the circulation.*
By M. BLAKE.

(Extracted from the Comptes Rendus.)

Solutions of several salts, potassa, soda, ammonia, baryte, lime, and magnesia, have been, says the author, injected into the veins or arteries, and the resulting phenomena have in most cases been studied with the assistance of the hæmodynamometer. A striking difference in the physiological action of these substances, has caused them to be divided into two classes; the one containing salts which destroy the irritability of the heart as soon as any blood vitiated by their presence

circulates in the partitions of this intestine ; and the other containing those substances which, without diminishing the irritability of the heart, cause death by stopping the blood in the lungs, by an influence which it seems to exercise over the capillary system of these organs. These two classes of substances, distinct as to their physiological action, are so also with regard to their chemical composition.

In fact, salts which have soda for a base seem to be the only ones which exercise no action on the irritability of the heart, whilst those of all other bases, at least all that we have tried, stop the contractions of the heart when they are introduced into the blood in any considerable quantity.

Our author goes on to say that, if the presence of the salts of soda in the blood do not stop the irritability of the heart, it determines other perturbations which cause these salts to be ranked as the most rapidly fatal poisons. If a solution of one of these substances be injected into the jugular vein of a dog, the arrival of the blood to the left heart, is hindered in about six seconds although the contractions of this entrail do not cease. At the same time the blood accumulates in the right heart and in the venous system, producing on the partitions of the veins a pressure capable of balancing a column of mercury two inches in length. This pressure re-acting on the sides of the ventricles of the brain, as on all the other parts of the venous system, must produce on the encéphale a degree of compression quite sufficient to account for the sudden death which happens, to animals subjected to experiment, thirty or forty seconds after the injection of the poison in the veins.

After death the heart still preserves its contractibility ; but so powerful is the obstacle which the capillaries of the lungs oppose to the passage of these substances over their calibers, that it has sometimes been impossible to find the slightest trace of them in the left heart. If the quantity of the salt introduced in the vein is not sufficient to completely stop the passage of the blood over the lungs, their action on the capillaries is still demonstrated by the augmentation of the bronchic secretion, of which the quantity is increased so as to cause the animal to perish of lethargy after having filled the aërial ways.

The phenomena which follow the injection of the second class salts in the veins are very different from those we have described above. The deepest method of studying their action, consists in injecting them in the veins of an animal whose thorax has previously been opened, and upon which the artificial respiration is practiced ; from seven to ten se-

conds after the injection, we perceive the movements of the heart cease, and the irritability of this entrail so completely destroyed, that however small the dose of poison has been, the application even of the two poles of the pile, some seconds after death, is insufficient to reproduce the contractions of the heart. Death does not follow with so much rapidity as when the pulmonary circulation is stopped, for we see the sensibility and respiration continue from two to three minutes, after the pulsations of the heart have ceased.

XXI.—*“Proceedings of the American Philosophical Society.”—November and December, 1839.*

The committee, consisting of Dr. Bache, Dr. Patterson, and Mr. Booth, to whom the paper of Doctor Hare, read at the last meeting of the society, was referred, entitled, “Description of an Apparatus for deflagrating carburets, phosphurets, or cyanides, in vacuo, or in an atmosphere of hydrogen, between electrodes of charcoal; with an account of the results obtained by these and other means, especially the isolation of calcium, and formation of a new fulminating compound, By R. Hare, M. D., Professor of Chemistry in the University of Pennsylvania,” reported in favor of publication in the Society’s Transactions. The publication was ordered accordingly.

The apparatus is of a convenient construction for the purposes designated in the title of the paper. The lower electrode or cathode is a parallelopipedon of charcoal, on which the body is placed, to be subjected to the influence of one or more batteries; and tubes with valve-cocks, communicating with an air pump, a barometer-gauge, and a reservoir of hydrogen, open into the interior of a ground plate, on which a bell-glass is fitted, air tight. In the experiments of the author, an equivalent of lime was heated with one equivalent and a half of bicyanide of mercury, in a porcelain crucible, enclosed in the alembic made for this purpose, and described in a former paper. The weight of the residue was such as would result from the union of an equivalent of calcium with an equivalent of cyanogen. This was then subjected to galvanic action on the cathode of the apparatus, the anode being brought in contact with it, and the result was the production of masses on the charcoal, having a metallic appearance.

Phosphuret of calcium, exposed in the same manner in the galvanic circuit, left pulverulent matter which effervesced

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in water, and, when rubbed on porcelain, appeared to contain metallic spangles, which were rapidly oxidized in the air.

In one experiment, particles of charcoal, apparently fused or resembling plumbago, dropped from the anode.

After heating lime with bityanide of mercury, the mass was dissolved in acetic acid, in which nitrate of mercury produced a copious white precipitate, that detonated under the hammer like fulminating silver.

On a New Compound of Deutochloride of Platinum, Nitric Oxide, and Hydrochloric Acid. By HENRY D. ROGERS, Professor of Geology in the University of Pennsylvania, and MARTIN H. BOYE, Graduate of the University of Copenhagen.

This substance is procured by dissolving platinum in an excess of nitromuriatic acid, and evaporating nearly to dryness; after which it is treated with aqua regia, freshly prepared, from concentrated hydrochloric and nitric acids. A little water is afterwards added, drop by drop, just sufficient to keep the chloride of platinum dissolved, when the compound will remain in the form of a gamboge yellow powder. It is then separated by decanting and filtering, and pressed between the folds of bibulous paper, and dried *in vacuo* over sulphuric acid.

The precipitate is a yellow, minutely crystalline powder, which absorbs water with great avidity. It may be preserved, without decomposition, in dry air, or *in vacuo*. It is decomposed by water, alcohol, &c., with extrication of nitric oxide, chloride of platinum remaining in solution. A concentrated solution of chloride of platinum has, however, no action on it. Heated in an atmosphere by hydrogen, it gives, off a large amount of chloride of ammonium, leaving a residuum of metallic platinum.

ANALYSIS.—The salt analysed, was prepared and kept in the manner described. Heated to the temperature of 212° F., it does not part with any of its water of combination. For estimating the amount of platinum and chlorine, the salt was fused with carbonate of potassa, &c., and the platinum, thus obtained, weighed by itself, and the chlorine precipitated from the solution by nitrate of silver.

The quantity of nitric oxide was determined by introducing a portion of the salt into a graduated tube, inverted over mercury, and decomposing it by letting up the requisite proportion of water.

The mean of a series of experiments, varied in different ways, gave

Platinum, -	41.26 per cent.
Chlorine, -	43.89 “
Nitric oxide	4.98 “

The above results correspond to five atoms of bichloride of platinum; five atoms of hydrochloric acid, and two atoms of nitric oxide. The water was calculated from the loss, in the analysis, to be equivalent to ten atoms.

Respecting the chemical nature of this compound, it may be regarded, either as a chloride of platinum, with a muriate of nitric oxide, represented by the following formula, $(\text{Pt Cl}^2)^5 + [(\text{Cl H})^5 + (\text{NO}^2)^2] + 10 \text{ Aq}$, or as a double chlorosalt, a chloroplatinate of nitrogen, with a chloroplatinate of hydrogen, represented by the formula, $[(\text{Pt Cl}^2)^2 + \text{N Cl}^2]^2 + (\text{Pt Cl}^2 + \text{H Cl}) + 14 \text{ Aq}$.

Hall of the American Philosophical Society.

PHILADELPHIA, December, 1839.

To the Hon. JOEL R. POINSETT, Secretary of War, &c. &c.

Sir:—The undersigned have been appointed a committee of the American Philosophical Society, to call your attention to, and invite, through the medium of your department, co-operation in, the extensive system of magnetic and meteorological observations about to be made under the direction of the British Government, and in connexion with their Antarctic expedition, particularly directed towards magnetic investigations.

The science of terrestrial magnetism has of late years made great advances, through the instrumentality of Humbolt, Hansteen, Gauss and others, and has now reached that point where a system of combined observations at widely distant points over the surface of the globe, appears to be necessary to its further progress: desultory effort has already done all that it is competent to effect. Such a series of systematic observations has now been set on foot by the British Government, directed to a better determination of the magnetic lines, for the use of navigators, and to the accurate investigation of the magnetic elements for theoretical purposes. The objects embraced are the measurement of the magnetic intensity, dip, and variation, at different stations, by a nautical expedition, and at fixed observatories, and especially the investigations of the variations of these elements at the latter points. As subsidiary to these objects, combined meteorological observations

are to be made, which cannot fail to elucidate some of the most important questions in this useful science.

The magnetic changes to be investigated are of three kinds : first, those which, depending upon a cause not yet satisfactorily explained, take place slowly but regularly, causing a general displacement of the lines of equal variation and dip ; secondly, those which, depending upon the position of the sun, run through their period of change in a year or day, producing different values in the magnetic elements, according to the season or to the hour of the day ; and thirdly, the small disturbances which appear to be constantly taking place, and which require for their measurement continued observation with the most accurate instruments.

The striking fact was proved in 1818, by the observations of Arago, at Paris, and of M. Kupffer, at Kasan, that the large changes which take place in the position of the horizontal needle during the day, are simultaneous at these places, so distant from each other ; and a confirmation of the fact as applying to even more distant stations, resulted from the system of observations established by Humboldt and others in 1830, and extended, through the influence of the Imperial Academy of Sciences of St. Petersburg, to the most remote parts of the Russian empire, and even to Peking. In 1834, the celebrated German philosopher Gauss, invented an instrument for measuring the variation of the needle and its changes, which introduced into these determinations an accuracy similar to that attainable in astronomical measurements. This instrument was soon furnished to different observatories, and a concerted system of observations of the minute changes of variation was introduced, which is now going on at no less than twenty-three places in Europe, the smaller and larger states having vied with each other in providing the means of executing them. The stations include Altona, Augsburg, Berlin, Bonn, Brunswick, Breda, Breslau, Cassel, Copenhagen, Cracow, Dublin, Freyberg, Göttingen, Greenwich, Halle, Kasan, Leipsic, Marburg, Milan, Munich, Naples, St. Petersburg, and Upsala.

The results already obtained and published by the German Magnetic Association, have proved satisfactorily that the minute changes in the direction of the needle, as well as the larger ones, are simultaneous at the different stations, varying however in amount, and the variation appearing to decrease in passing southward ; but the influence of the position of the place, whether depending upon geographical or magnetic position, not having yet been fully determined, and being probably determinable only by observations at places even

more distant from each other than those now embraced in the German series.

The invention of an instrument by Gauss, for determining the changes in horizontal magnetic intensity with the same accuracy as those of the direction of the needle, will give rise to interesting developments in regard to them; and the changes of the three elements of horizontal direction, and horizontal and vertical intensity are all included by the two instruments before referred to, and a third invented by Professor Lloyd, of Dublin. It is the object of the series now projected, to embrace these three elements; to extend the number of stations with special reference to their distribution at points of the earth interesting in their magnetic relations; to keep up a constant series of simultaneous observations for three years; and thus to effect, on an extended scale, what the German Magnetic Association has so well begun. The execution of this plan, with observations of an appropriate kind, directed also to magnetic research, by a naval expedition, was recommended to the British Government by the members of the British Association, including men of science from different countries, in 1838. It subsequently received the sanction of the Royal Society of London, was adopted by the Government, and is now in course of execution. It may be considered, therefore, to have been approved by the highest scientific authorities. In pursuance of this plan, stationary observatories are to be established, and regular observations made, for the next three years, at Toronto in Upper Canada, at St. Helena, at the Cape of Good Hope, and at a station in Van Dieman's Land. The East India Company have also undertaken to furnish the means of observation at nine points in their dominions. European Governments, who have not hitherto joined in the German system, with which this will be in connexion, have also promised similar aid. It is this extended scheme, to which our attention has been specially invited by circular from the Royal Society of London, and in which the American Philosophical Society desires that our country should co-operate. It is on a broad scale, worthy of all encouragement, and the magnitude of the scheme, the objects for which it is undertaken, and the possibility of its execution, all mark the character of the period in which we live.

The Society would propose, in furtherance of this plan, that five magnetic observatories should be established in the N. E., N. W., S. E., S. W., and at some central point of the United States, furnished with the instruments and observers necessary, fully to carry out the proper plan of combined

magnetic and meteorological observations. Should the proposition to make this co-operation truly national, be acceded to, the details in relation to it can easily be arranged, and the Society will, the undersigned confidently believe, feel proud, to lend any aid in their power, in planning or executing them. It may perhaps be more satisfactory however, to state briefly, beforehand, the nature of the observations to be made, and the means required for their execution.

The magnetic observations to be undertaken at the fixed observatories are, first, of the variation (declination), absolute horizontal intensity and dip; second, of the changes of the variation of the horizontal intensity, and of the vertical intensity. The regular observations for changes in these elements, are to be made every two hours every day, (with the exception of Sundays,) for the next three years, beginning as soon as the several observatories can be arranged. To these are to be added more frequent observations on one day of each month, including the four terms during the year, fixed by the German Magnetic Association. At each station, a building of stone or wood will be required, in the construction of which no iron must be employed. The instruments adopted by the British observers are the following: A magnetometer for the declination, one for the horizontal force, one for the vertical force, a dipping needle, azimuthal transit, two reading telescopes, and two chronometers. The estimated cost of each set of these, is about fourteen hundred dollars. The cost of the observatory must vary with the place at which it is erected, and the material chosen for it, but may be estimated at from one thousand to fifteen hundred dollars. One principal and three assistants will suffice for making and reducing the observations at each station, and for carrying on a supplementary series of meteorological observations. The meteorological observations proposed, are on the pressure, temperature, and moisture of the air; on the direction and force of the wind; on the quantity of rain; on the temperature of the ground at different depths; on solar and terrestrial radiation; besides a few miscellaneous and occasional observations, not necessary to be here stated. Regular observations are to be made on these points, four times every day, and every hour on one day in each month. The instruments required at each station, are a barometer, a standard thermometer, a maximum and minimum thermometer, a hygrometer, an anemometer, several extra thermometers, an actinometer, and an apparatus for atmospheric electricity. The probable cost of each set of these would not exceed two hundred and fifty dollars. The value of the results would be much increased,

by providing a self-registering anemometer and rain-gauge, instead, of the common ones, which would increase the cost of each set of instruments to five hundred and seventy dollars. The whole cost of erecting the five observatories, and providing them with excellent instruments, will probably not exceed sixteen thousand dollars; and if the observatory already existing at Philadelphia, and provided with the necessary instruments, should be adopted as one of the five, and four others be erected and furnished, the expense to the United States would not exceed twelve thousand dollars.

No estimate is made of the cost of the principal and assistants for the proposed observatories. In the organization of the new British stationary observatories, these persons are taken, in part, if not altogether, from the officers, non-commissioned officers, and privates of the artillery. The acquirements of the graduates of our Military Academy, admirably fit them for directing the observatories, which might be appropriately placed at military posts; so as to provide the officers and men necessary for making the observations, without additional expense. The direction thus given to the views of the committee; the fact that you have long been enrolled as a member of the American Philosophical Society; and the interest which you have always manifested, both as an individual and in a public capacity, in all enterprises calculated to shed a lustre upon your country, have induced the Society to direct us to address ourselves particularly to you on this subject.

With the hope that your views may coincide with those of the Society, in regard to the plan now presented for your consideration, we are, very respectfully, yours,

A. D. BACHE,	} Committee.
R. M. PATTERSON,	
JOSEPH HENRY,	
J. K. KANE,	
JOS. G. TOTTEN,	

On the Congelation of Water by the Evaporation of Ether.
By Dr. HARE.

For effecting the congelation of water by the evaporation of ether, it had been usual to expose a bulb, containing water and moistened by the ether, to a current of air. Recently Dr. Hare had succeeded far more satisfactorily by exposing a quantity of water, twenty times as large as that usually employed, covered by ether in a capsule to a blast of air, proceeding from a vessel in which it had been condensed by a

pressure equal to one or two atmospheres. By these means, the freezing of the water might be seen by five hundred spectators.

Having mentioned that the pure hyponitrous ether recently obtained, caused a cold of 15° by its evaporation, it would of course be inferred, as he had found to be the fact, that this last mentioned ether might be advantageously employed.

When hydric ether is employed, it should not exceed 730 in specific gravity.

Dr. Hare further said, that it would probably be remembered, that about two years since, he had published an account of a new process for freezing water by the evaporation of ether, caused by a diminution of atmospheric pressure. In the process then described, concentrated sulphuric acid was interposed between the retort holding the water and ether, and the air pump. Since that time he had rendered the process more rapid and interesting by interposing an iron mercury bottle, with two cocks between the receiver holding the acid and the pump. The ether and water were introduced into the retort. The beak of the retort, properly bent, entered the receiver, through the tubulure to which it was luted. The beak was of such a length and curvature, as to cause its orifice to be below the surface of the acid. The neck of the receiver communicated with the cavity of the bottle, that of the bottle with the pump. The apparatus being thus arranged, the bottle was exhausted, and the cock, communicating with the pump, closed. Under these circumstances, on opening a communication between the bottle and receiver, the pressure in that vessel and in the retort was so much reduced as to cause the instantaneous ebullition of the ether, so that little, if any subsequent aid, was required from the pump. But the result which gave increased interest to the process, was the inconceivable rapidity with which the acid, under these circumstances, absorbed the ethereal vapor, which it appeared to do with greater avidity as the process advanced.

In fact, the water, in the act of congealing, flew all over the inner surface of the retort, in consequence of an explosive evolution of ethereal vapor, generated amid the aqueous particles. The congelation of the water was rendered evident to the ears as well to the eyes of his class of more than three hundred students.

Dr. Hare said, it did not appear to him that sufficient attention had been paid by artists or men of science, to the great difference which existed between the effect upon glass of heating it by radiation and by conduction. When exposed to radiant heat alone, unaccompanied by flame, or a current

of hot air, glass is readily penetrated by it, and is heated, within and without, with commensurate rapidity; but in the case of its exposure to an incandescent vapour or gas, the caloric could only penetrate by the process of conduction; and, consequently, from the inferior conducting power of glass, the temperature of the outer and inner portions of the mass would be so different, as by the consequent inequality of expansion to cause the fracture, which was well known, under such circumstances, to ensue.

The combustion of anthracite coal, in an open grate, in his laboratory, having four flues of about 4.12 by 2.12 inches each, in area, just above the level of the grate (the upper stratum of the fire, having nothing between it and the ceiling,) had allowed him to perform some operations with success, which formerly he would have considered impracticable. The fire having attained to that state of incandescence to which it easily arrives when well managed, he had, on opening a hole by means of an iron rod, so as to have a perpendicular perforation extending to the bottom of the fire, repeatedly fused the beaks of retorts of any capacity, not being more than three gallons, causing them to draw out, by the force of gravity, into a tapering tube; so that, on lifting the beak from the fire, and holding the body of the retort upright, the fused portion would hang down so as to form an angle with the rest of the beak, or to have any desired obliquity. By these means, in a series of retorts, the beak of the first might be made to descend through the tubulure of a second; the beak of the second through that of a third, and so on; the beak of the last retort in the row being made, when requisite, to enter a tube passing through ice and water in an inverted bell-glass.

By means of the anthracite fire, as above described, thick rods, as well as stout tubes, might, as he had found, be softened and extended, or bent into suitable forms.

The lower end of a green glass phial, such as is used usually for Cologne water, might be made to draw out into a trumpet-shaped extremity. A Florence flask might be heated, and made flat, so as to answer better for some purposes. The drawing out of tubes into a tapering form, suitable for introducing liquids through retort tubulures, was thus easily effected; and in all cases the sealing of large tubes was better commenced in this way, although the blowpipe might be necessary to close a capillary opening which could not be closed by the fire.

Dr. Hare further communicated a method of preparing pure chlorohydric acid, from the impure muriatic acid of commerce, by the action of sulphuric acid.

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It is known, said Dr. Hare, that concentrated sulphuric acid, when added to liquid chlorohydric acid, expels more or less of it as a gas, in consequence of its superior affinity for water. At the present low price of the ordinary acid of commerce, Dr. Hare had found it advantageous to procure the latter in purity, by subjecting it to the former.

A tubulated glass retort, having been half filled with chlorohydric acid, sulphuric acid was allowed to drop from a glass funnel, with a cock, into a tube descending into the acid in the retort through the tubulure, to which it was luted by strips of gum-elastic. The tube terminated in a very small bore. The beak of the retort, bent in the fire, as he had just described, descended through the tubulure into the body of a small retort containing water not refrigerated. The beak of the latter descended into a larger one, half full of water, to which ice was applied. Of course the beak of the third might, in like manner, enter the body of a fourth. After an equivalent weight of sulphuric acid had been introduced, and the evolution of gas was no longer sufficiently active, heat might be applied until nearly all the chlorohydric acid should come over.

The residual diluted sulphuric acid was, with the addition of nitrate of soda or potassa, or nitric acid, as serviceable for galvanic purposes, as if it had not been thus used.

Dr. Hare further communicated a method of preparing hydrochloric acid and chlorine in the self-regulating reservoir invented by him, and spoke of some of the applications of the gases thus prepared.

Dr. Hare was under the impression that few chemists were aware of the great advantage of the self-regulating reservoirs of gas, to which he had resorted. He was enabled, by means of them, to keep hydrogen, carbonic acid, nitric oxide, chlorine, chlorohydric acid, sulphydric acid, and arseniuretted hydrogen, so as to use any of these gases at pleasure. He had kept these reservoirs in operation for months, without taking the constituent vessels apart.

By means of the reservoir of chlorohydric acid he had been encouraged to make an effort which proved successful; to form artificial camphor by the impregnation of oil of turpentine with that gas.

Subjecting an ingot of tin to a current of chlorine from his reservoir, it was rapidly converted into the bichloride, or fuming liquor of Libavius. To his surprise the ingot was fused by the heat generated. In the last mentioned reservoir, the materials were manganese, in lumps, and concentrated chlorohydric acid, diluted sulphuric acid being also introduced; as

the reaction of this last mentioned acid with the manganese was more active than that of the chlorohydric acid. In fact, sulphuric acid, diluted with its weight of water and common salt, might be used without chlorohydric acid. In the reservoir for chlorohydric acid, the materials were sal ammoniac and sulphuric acid, to which some water was added, but not so much as to prevent the chlorohydric acid from assuming the gaseous state.

He had found it preferable to keep the sulphydric acid reservoir in a flue, the gas being drawn, when wanted, through a globe of water, by means of a leaden tube, at a convenient place. It would be desirable that the reservoirs of chlorine and chlorohydric acid should be similarly situated.

XXII.—MISCELLANEOUS ARTICLES.

Wreck of the "Royal George."

The great explosion announced on the 16th instant took place on the 22nd, being the same day, and very little later than the hour stated. The effect was beautiful, and the intention of firing it having been generally known, it was witnessed by a vast number of spectators, notwithstanding that it blew a stiff breeze, which deterred many from going out to Spithead. The morning was very fine, but doubts were entertained whether the high wind might not prevent the operation, until eight o'clock, when the red flag was hoisted from all the Royal George flotilla—namely, the Success frigate hulk, and the two lumps usually moored over the wreck, one of which, No. 4, was removed about 60 yards to the westward, whilst the other remained over the spot intended for the explosion. As soon as these flags were seen, it was known that the operation would be attempted, at the afternoon slack. In the mean time, Lieut. Symonds, the executive engineer, according to a plan preconcerted between him and Colonel Pasley, sent down Mr. George Hall, the diver, who placed first a charge of 47lb., and afterwards another of 260lb. of powder, on the spot originally occupied by the main hatchway on the orlop deck, which were fired successively by Professor Daniell's voltaic battery, as soon as he came up, the second charge being placed in the hole made by the first. The object of these charges, which were fired at the morning slack, was to make a deep crater or hole for the great charge proposed to be fired at the afternoon slack. Colonel Pasley came out about one o'clock, and at half-past one the great cylinder, loaded with 25½ barrels, or nearly 2,300lb. of gunpowder, with the voltaic con-

ducting apparatus attached to it, was raised out of a launch alongside by the derrick of No. 5, lump, and lowered into the water so as to rest a little above the surface, where it remained suspended by the bull-rope of the derrick. Hall was then sent down, and made fast a down haul rope with a single sheave block to a solid piece of timber, which he found at the bottom of the crater produced by the morning's explosion. He came up, and handed over the end of this rope, which was attached to the cylinder, to which a couple of pigs of ballast were added, to make it sink more easily; after which it was lowered from No. 5 lump, and accompanied in its descent by Hall, who had a line attached to it in his hand, and who made signals to the men above, either to lower or occasionally to raise, or to move the cylinder to the eastward or westward, as required, until he guided it into its proper place, where he lashed it to the timber before-mentioned. At about a quarter past two o'clock he came up, and reported that it was properly placed. Whilst being lowered, the voltaic conducting apparatus attached to the cylinder was veered out, and the other end of it was taken on board No. 4 lump, and placed near the voltaic battery, where Lieutenant Symonds now stationed himself. No. 5 lump was then removed to the distance of 70 or 80 yards to the southward of the spot where the cylinder had been let down. All being now ready, Colonel Pasley, who remained in that lump, ordered his bugler first to sound the "preparative," and in about a minute afterwards "the fire." At that moment Lieut. Symonds completed the circuit with the voltaic battery, and an immediate explosion took place, the shock being felt and the report heard at the same instant. In a few seconds afterwards the surface rose three or four feet in a circle of moderate size, from the center of which almost immediately afterwards, a splendid column of water at least 50 feet high, and of a conical form, was thrown up, beautifully sparkling in the sun, which was hailed by the hearty cheers of all the workmen employed, as well as the numerous spectators, and soon after several large fragments of the wreck came floating up to the surface, which proved to be the lower part of the main-mast. The form of the column of water was not so regular as on former occasions, owing to the strong wind which acted upon it. When it fell down again, clear circular waves spread outwards from the same centre, making a great commotion at the surface, and causing No. 4 lump, which was nearest to the explosion, to pitch a good deal. Soon after this the mud from the bottom came up, blackening the same circle of water, which spread outwards, discoloring the surface as it extended, and at the same time stilling the

swell of the sea for a space of perhaps 200 yards in diameter. A great number of small fish came up dead, as on former occasions, which were picked up by the boatmen. More than 50 yachts or large sailing boats came out, whose decks were covered with spectators, among whom were a great number of ladies. The two admirals and the general commanding the garrison, with a great number of naval and military officers, and most of the officers of the dockyard, with their families, were present, many of whom went on board the two lumps to have a better view of the operations. The deck of the Success frigate was also crowded with spectators. The Bishop of Norwich, the Astronomer Royal, and the Russian Consul-General were present.

The cylinder used on the 22nd was of wood with iron hoops, like a mooring buoy, made by Mr. Harding, the master capstan maker, in Chatham dockyard, and protected by two coats of canvass and several coats of a waterproof composition discovered by Sergeant-Major Jones, which by numerous experiments tried to compare it with other compositions, by order of Colonel Pasley, was found to be far superior to any in former use, as it combines absolute resistance to the greatest pressure of water with a certain degree of elasticity that does not allow it to crack. This skilful and zealous officer and a party of Royal sappers and miners have been most useful in all the operations, and since one of the three excellent professional divers engaged was obliged to give up on account of ill-health, Corporal Harris has supplied his place, and made himself much more useful in that capacity than could have been expected from so short an apprenticeship, for he has only worked two weeks in this department. Besides the divers, whose services are of the most essential importance, the dockyard riggers under Mr. Clewitt, of Portsmouth, and James Chapman, of Chatham yard, have given the greatest satisfaction, as well as the naval pensioners, about 40 in number, most of whom were petty officers, and who, though all middle-aged or elderly men, have been extremely zealous and efficient. The Lively sailing lighter, commanded by Mr. Harfield, goes backwards and forwards continually, and the seamen belonging to her are kept in constant employment in taking on board or landing the timbers and guns recovered from the wreck, of the former of which an immense pile has been deposited in Portsmouth dockyard. The whole is superintended by Lieut. Symonds with great skill and indefatigable activity, who carries on the work at every slack tide, except when it blows a gale approaching to a hurricane, that is, always twice, and sometimes three times, a day, according to the moon's age, and never goes on shore

but on Sundays. Col. Pasley usually goes out to Spithead once a day, except when his duty requires him at Chatham, where he passes about half his time. The massy oak timbers upon which the foremast and mizenmast were stepped, the after-part of the keel, the whole sternpost and dead wood over it, and an immense mass of the starboard bow, which rested on the keel, weighing more than 30 tons, and which was knocked down about eight months ago by the last great explosion of 1839, having been got up this season, together with the heel of the mainmast having been disengaged from its step by the great explosion which we have just described—these circumstances shew that the demolition and removal of wreck have extended nearly to the bottom of the vessel, and also prove that the mud has no tendency to accumulate, but is cleared away by the action of the tides, in proportion as the upper parts of the wreck are demolished and removed. The effect of the last explosion could not be ascertained, as the tide ran too strong for Hall, the diver, who went down again after it, to quit his ladder, but it is presumed that it will have blown out the larboard side of the wreck, and that it will have broken up timbers in all directions, probably fracturing not only the step of the mainmast, but the keelson and keel below it. The starboard side opposite was shattered and thrown down by the first great explosion of 1839. Upon the whole, though opinions were very much divided at first, and perhaps generally unfavorable, there seems every reason to hope, that before the end of the season the whole of the wreck of the Royal George will have been sufficiently removed to enable vessels to anchor over the spot without incurring the risk of losing their anchors and cables. If so, probably the Government may be induced to remove the wreck of the Edgar also, from which Lieut. Symonds recovered five iron guns during Col. Pasley's last official visit to Chatham, the surface of which, after an immersion of 129 years, proved to be converted into very soft carburet of iron or plumbago to a considerable depth, on removing the mass of oysters, &c., with which they were incrustated. These guns are remarkable for being much thicker at the breech and thinner at the muzzle than guns of the same calibre of more modern construction, from which the iron guns recovered from the Royal George differ very little.

The Atmospheric Railway Carriages.

It is upwards of a year and a half since we described the experiment made by Mr. Clegg, in the borough, on the mode of propelling carriages by means of exhausting a pipe, or tube,

with which the carriages were connected, and then admitting the atmospheric air, and, as it were, forcing the carriages or train along, and thus superseding the necessity of gas or any other power for the conveyance of a train along a rail or tram-road. These experiments were very successful; indeed, so successful, that few persons had any doubt about the ultimate success of the principle of the invention. On Thursday the experiments were exhibited on a large scale, on a railroad which connects, or is intended to connect, the line of the Birmingham, the Bristol, and the Thames Junction Lines, commencing within a short distance of Shepherd's Bush, and running in a westerly direction for about three quarters of a mile. The carriage put in motion, with the persons on it, did not weigh much less than 12 tons; but it travelled with great ease at the rate of 25 miles an hour. The exhaustion pipe, or tube, laid down, which was not the propelling agent, but the means of development, was about 9 inches indiameter. The engine by which it was exhausted—namely, a pump, worked by steam—rendered it fit for the operations required in about two minutes and a half; and from the index, that is quicksilver, employed at the *termini*, it was ascertained that the operation was performed simultaneously at both ends of the line. There was no noise, no smoke, and, what is better, no danger of explosion, or of a power which could not be governed. In short, the experiment was as successful as its most warm well-wishers could expect, and shewed that the agency of steam is not a *sine quâ non* on a railroad. Without going into the minute history of railroads, it may be as well to say, that this power may be applied to any railroad at a saving of about 70 per cent.; and that it is of sufficient force to preclude the necessity of tunnelling. It is applicable to almost any gradients. The experiments were attended by a great number of the nobility, and by many scientific men. The Archbishop of Canterbury, Lords Charleville, Rodstock, and Prudhoe, the Marquis of Douro, Lord Howick, and Lord Stuart de Rothsay, &c. were on the ground, and expressed their satisfaction at the result of the experiments.—*Times*.

Frog found in Coal.

On Wednesday morning, as two colliers, George Ross and James Gardner, were in one of the rooms of the Old Muirfield Pit, at Gargieston, they found a living frog embodied in the solid seam of coal, at least twelve fathoms beneath the surface of the earth. The nich in which it had lived was perfectly smooth inside, of the exact shape of the frog, and

without a crack or crevice to give admittance to air. The hind legs of the animal are at least a third longer than usual, the fore legs shorter, the toes longer and harder, and its general colour is of a bronze hue. It leaped briskly about, the moment that it was excavated from its narrow cell. How many centuries it has been shut out from light and air, and entombed in its dreary dormitory, it is impossible to say—certain it is, that, although diminutive in form, and with great brilliancy of eye, it has a most antediluvian aspect.—*Scotch paper.*

On the separation of Lime from Magnesia, and on the Assay of Gold. By LEWIS THOMPSON, Esq. M. R. C. S.

To separate Lime from Magnesia.

Dissolve the combined earths in dilute nitric or muriatic acid, and precipitate the filtered solution by means of an excess of carbonate of soda; dry the precipitate, and place it in a coated green glass tube, so disposed that the whole can be heated to a dull red heat; when red hot pass a current of well-washed chlorine through the tube for a few minutes: the lime will be converted into chloride of calcium, but the magnesia remains unacted upon. When the whole is cool, remove the mass from the tube and boil it for a minute or two in water, filter the liquid and wash the insoluble portion (which is magnesia) with water, and precipitate the lime from the mixed liquors by carbonate of soda. The heat should not exceed a dull red, as the mass is apt to become vitrified at the part which touches the tube, and this renders it difficult to remove the contents.

To Assay Gold.

Take six grains of the gold to be assayed and place in a small crucible, with fifteen grains of silver, and from eight to twelve grains of chloride of silver, according to the supposed impurity of the gold; lastly add fifty grains of common salt (chloride of sodium) reduced to fine powder so as to prevent decrepitation; fuse the whole together for five minutes, and allow it to become cold; then take out the metallic button and beat it into a thin plate, and subject it to the action of dilute nitric acid as in the ordinary mode of parting. By this plan the tedious process of cupellation is avoided, the baser metals being wholly removed by the chlorine of silver, and their place supplied by pure silver.

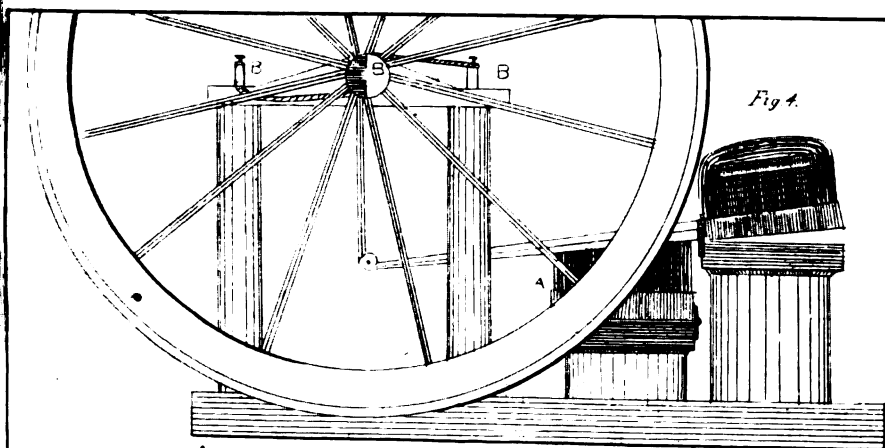


Fig. 1.

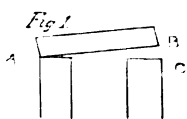


Fig. 2.

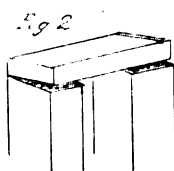


Fig. 3.

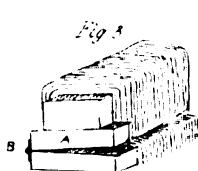


Fig. 4.

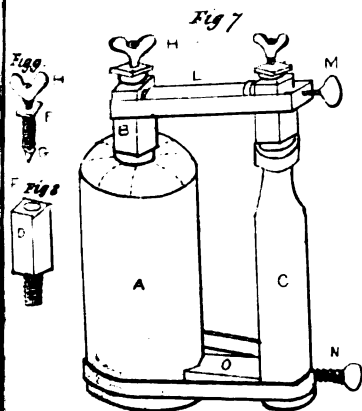


Fig. 5.



Fig. 6.

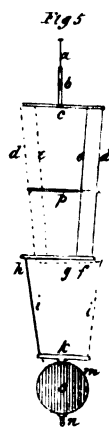


Fig. 7.

THE ANNALS
OF
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AND
Guardian of Experimental Science.

SEPTEMBER, 1840.

XXIII.—*Experimental Researches in Electricity.—Twelfth Series.* By MICHAEL FARADAY, Esq. D. C. L. F. R. S. Fullerman Prof. Chem. Royal Institution, Cor. Memb. Royal and Imp. Acad. of Sciences, Paris, Petersburg, Florence, Copenhagen, Berlin, &c. &c.

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¶ ix. *Disruptive discharge continued—Insulation—Spark—Brush—Difference of discharge at the positive and negative surfaces of conductors.*

1392. When a spark had passed at either interval, then, generally, more tended to appear at the *same* interval, as if a preparation had been made for the passing of the latter spark. So also on continuing to work the machine quickly the sparks generally followed at the same place. This effect is probably due in part to the warmth of the air heated by the preceding spark, in part to dust, and I suspect in part to something unperceived as yet in the circumstances of discharge.

1393. A very remarkable difference, which is *constant* in its direction, occurs when the electricity communicated to the balls *s* and *S* is changed from positive to negative, or in the contrary direction. It is that the range of variation is always greater when the small balls are positive than when they are

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negative. This is exhibited in the following Table, drawn from the former experiments.

	Pos.	Neg.
In Air the range was . . .	0.19	0.09
Oxygen	0.19	0.02
Nitrogen	0.13	0.11
Hydrogen	0.14	0.05
Carbonic acid	0.16	0.02
Olefiant gas	0.22	0.08
Coal gas	0.24	0.12
Muriatic acid	0.43	0.08

I have no doubt these numbers require considerable correction, but the general result is striking, and the differences in several cases very great.

1394. Though, in consequence of the variation of the striking distance (1386.), the interval in air fails to be a measure, as yet, of the insulating or resisting power of the gas in the vessel, yet we may for present purposes take the mean interval as representing in some degree that power. On examining these mean intervals as they are given in the third column (1388.), it will be very evident, that gases, when employed as dielectrics, have peculiar electrical relations to insulation, and therefore to induction, very distinct from such as might be supposed to depend upon their mere physical qualities of specific gravity or pressure.

1395. First, it is clear that at the *same pressure* they are not alike, the difference being as great as 37 and 110. When the small balls are charged positively, and with the same surfaces and the same pressure, muriatic acid gas has three times the insulating or restraining power (1362.) of hydrogen gas, and nearly twice that of oxygen, nitrogen, or air.

1396. Yet it is evident that the difference is not due to specific gravity, for though hydrogen is the lowest, and therefore lower than oxygen, oxygen is much beneath nitrogen, or than olefiant gas, and carbonic acid gas, though considerably heavier than olefiant gas or muriatic gas, is lower than either. Oxygen as a heavy, and olefiant as a light gas, are in strong contrast with each other; and if we may reason of olefiant gas from HARRIS's results with air (1365.), then it might be rarefied to two-thirds its usual density, or to a specific gravity of 9.3 (hydrogen being 1), and having neither the same density, nor pressure as oxygen, would have equal insulating powers with it, or equal tendency to resist discharge.

1397. Experiments have already been described (1291.

1292.) which shew that the gases are sensibly alike in their inductive capacity. This result is not in contradiction with the existence of great differences in their restraining power. The same point has been observed already in regard to dense and rare air (1375.).

1398. Hence arises a new argument proving that it cannot be mere pressure of the atmosphere which prevents or governs discharge (1377. 1378.) but a specific electric quality or relation of the gaseous medium. Hence also additional argument for the theory of molecular inductive action.

1399. Other specific differences amongst the gases may be drawn from the preceding series of experiments, rough and hasty as they are. Thus the positive and negative series of mean intervals do not give the same differences. It has been already noticed that the negative numbers are lower than the positive (1393.), but besides that, the *order* of the positive and negative results is not the same. Thus on comparing the mean numbers (which represent for the present insulating tension,) it appears that in air, hydrogen, carbonic acid, olefiant gas, and muriatic acid, the tension rose higher when the smaller ball was made positive than when rendered negative, whilst in oxygen, nitrogen, and coal gas, the reverse was the case. Now though the numbers cannot be trusted as exact, and though air, oxygen, and nitrogen should probably be on the same side, yet some of the results, as, for instance, those with muriatic acid, fully shew a peculiar relation and difference among gases in this respect. This was further proved by making the interval in air 0.8 of an inch whilst muriatic acid gas was in the vessel *a*; for on charging the small balls *s* and *S* positively, *all* the discharge took place through the *air*; but on charging them negatively, *all* the discharge took place through the *muriatic acid gas*.

1400. So also, when the conductor *n* was connected *only* with the muriatic acid gas apparatus, it was found that the discharge was more facile when the small ball *s* was negative than when positive; for in the latter case, much of the electricity passed off as brush discharge through the air from the connecting wire *p*; but in the former case, it all seemed to go through the muriatic acid.

1401. The consideration, however, of positive and negative discharge across air and other gases will be resumed in the further part of this, or in the next paper.

1402. Here for the present I must leave this part of the subject, which had for its object only to observe how far gases agreed or differed as to their power of retaining a charge on bodies acting by induction through them. All the

results conspire to shew that Induction is an action of contiguous molecules (1295. &c.); but besides confirming this, the first principle placed for proof in the present inquiry, they greatly assist in developing the specific properties of each gaseous dielectric, at the same time shewing that further and extensive experimental investigation is necessary, and holding out the promise of new discovery as the reward of the labour required.

1403. When we pass from the consideration of dielectrics like the gases to that of bodies having the liquid and solid condition, then our reasonings in the present state of the subject assume much more of the character of mere supposition. Still I do not perceive anything adverse to the theory in the phenomena which such bodies present. If we take three insulating dielectrics, as air, oil of turpentine and shell-lac and use the same balls or conductors at the same intervals in these three substances, increasing the intensity of the induction until discharge take place, we shall find that it must be raised much higher in the fluid than for the gas, and higher still in the solid than for the fluid. Nor is this inconsistent with the theory; for with the liquid, though its molecules are free to move almost as easily as those of the gas, there are many more particles introduced into the given interval; and as respects the latter circumstance, the same is the case when the solid body is employed. Besides that, the cohesive force of the body used will produce some effect; for though the production of the polarized states in the particle of a solid may not be obstructed, but, on the contrary, may in some cases be even favored (1164. 1344.) by its solidity or other circumstances, yet solidity may well exert an influence on the point of its final subversion, (just as it prevents discharge in an electrolyte,) and so enable inductive intensity to rise to a much higher degree.

1404. In the cases of solids and liquids too, bodies may, and most probably do, possess specific differences as to their ability of assuming the polarized state, and also as to the extent to which that polarity must rise before discharge occurs. An analogous difference exists in the specific inductive capacities already pointed out in a few substances (1278.) in the last paper. Such a difference might even account for the various degrees of insulating and conducting power possessed by different bodies, and, if it should be found to exist, would add further strength to the argument in favor of the molecular theory of inductive action.

1405. Having considered these various cases of sustained insulation in non-conducting dielectrics up to the highest point which they can attain, we find that they terminate at last in *disruptive discharge*; the peculiar condition of the molecules of the dielectric which was necessary to the continuous induction, being equally essential to the occurrence of that effect which closes all the phenomena. This discharge is not only in its appearance and condition different to the former modes by which the lowering of the powers was effected (1320. 1343.), but, whilst really the same in principle, varies much from itself in certain characters, and thus presents us with the forms of *spark*, *brush* and *glow* (1359.). I will first consider *the spark*, limiting it for the present to the case of discharge between two oppositely electrified conducting surfaces.

The electric spark or flash.

1406. The *spark* is a discharge or lowering of the polarized inductive state of many dielectric particles, by a particular action of a few of the particles occupying a very small and limited space; all the previously polarized particles returning to their first or normal condition in the inverse order in which they left it, and uniting their powers meanwhile to produce, or rather to continue, (1417 and 1436.) the discharge effect in the place where the subversion of force first occurred. My impression is, that the few particles situated where discharge occurs are not merely pushed apart, but assume a peculiar state, a highly exalted condition for the time, i. e. have thrown upon them all the surrounding forces in succession, and rising up to a proportionate intensity of condition, perhaps equal to that of chemically combining atoms, discharge the powers, possibly in the same manner as they do theirs, by some operation at present unknown to us; and so the end of the whole. The ultimate effect is exactly as if a metallic wire had been put into the place of the discharging particles; and it does not seem impossible that the principles of action in both cases may, hereafter, prove to be the same.

1407. The *path of the spark*, or of the discharge, depends on the degree of tension acquired by the particles in the line of discharge, circumstances, which in every common case are very evident and by the theory easy to understand, rendering it higher in them than in their neighbours, and, by exalting them first to the requisite condition, causing them to determine on the course of the discharge. Hence the selection of the path, and the solution of the wonder which Harris has so

well described* as existing under the old theory. All is prepared amongst the molecules beforehand, by the prior induction, for the path either of the electric spark or of lightning itself.

1408. The same difficulty is expressed as a principle by Nobili for voltaic electricity, almost in Mr. Harris's words, namely,† “electricity directs itself towards the point where it can most easily discharge itself,” and the results of this as a principle he has well wrought out for the case of voltaic currents. But the *solution* of the difficulty, or the proximate cause of the effects, is the same: induction brings the particles up to or towards a certain state (1370.); and by those which first attain it, is the discharge first and most efficiently performed.

1409. The *moment* of discharge is probably determined by that molecule of the dielectric which, from the circumstances, has its tension most quickly raised up to the maximum intensity. In all cases where the discharge passes from conductor to conductor this molecule must be on the surface of one of them; but when it passes between a conductor and a non-conductor, it is, perhaps, not always so (1453.). When this particle has acquired its maximum tension, then the whole barrier of resistance is broken down in the line or lines of inductive action originating at it, and disruptive discharge occurs (1370.): and such an inference, drawn as it is from the theory, seems to me in accordance with Mr. Harris's facts and conclusions respecting the resistance of the atmosphere, namely, that it is not really greater at any one discharging distance than another.‡

1410. It seems probable, that the tension of a particle of the same dielectric, as air, which is requisite to produce discharge, is a *constant quantity*, whatever the shape of the part of the conductor with which it is in contact, whether ball or point; whatever the thickness or depth of dielectric throughout which induction is exerted; perhaps, even, whatever the state, as to rarefaction or condensation of the dielectric; and whatever the nature of the conductor, good or bad, with which the particle is for the moment associated. In saying so much, I do not mean to exclude small differences which may be caused by the reaction of neighbouring particles on the deciding particle, and indeed, it is evident that the intensity required in a particle must be related to the

* Nautical Magazine, 1834, p. 229.

† Bibliotheque Universelle, 1835. lix. 275.

‡ Philosophical Transactions, 1834, pp. 227, 229.

condition of those which are contiguous. But if the expectation should be found to approximate to truth, what a generality of character it presents! and, in the definiteness of the power possessed by a particular molecule, may we not hope to find an immediate relation to the force which, being electrical, is equally definite and constitutes chemical affinity?

1411. Theoretically it would seem that, at the moment of discharge by the spark in one line of inductive force, not merely would all the other lines throw their forces into this one (1406.), but the lateral effect, equivalent to a repulsion of these lines (1224. 1297.), would be relieved and, perhaps, followed by something equivalent to a contrary action, amounting to a collapse or attraction of these parts. Having long sought for some transverse force in statical electricity, which should be the equivalent to magnetism or the transverse force of current electricity, and conceiving that it might be connected with the transverse action of the lines of inductive force already described (1297.), I was desirous, by various experiments, of bringing out the effect of such a force, and making it bear upon the phenomena of electro-magnetism and magneto-electricity.

1412. Amongst other results, I expected and sought for the mutual affection, or even the lateral coalition of two similar sparks, if they could be obtained simultaneously side by side, and sufficiently near to each other. For this purpose, two similar Leyden jars were supplied with rods of copper projecting from their balls in a horizontal direction, the rods being about 0.2 of an inch thick, and rounded at the ends. The jars were placed upon a sheet of tinfoil, and so adjusted that their rods, *a* and *b*, were near together, in the position represented in plan at fig. 2. *c* and *d* were two brass balls connected by a brass rod and insulated; *e* was also a brass ball connected, by a wire, with the ground and with the tinfoil upon which the Leyden jars were placed. By laying an insulated metal rod across from *a* to *b*, charging the jars, and removing the rod, both the jars could be brought up to the same intensity of charge (1370.). Then, making the ball *e* approach the ball *d*, at the moment the spark passed there, two sparks passed between the rods *n*, *o*, and the ball *c*; and as far as the eye could judge, or the conditions determine, they were simultaneous.

1413. Under these circumstances two modes of discharge took place; either each end had its own, particular spark to the ball, or else one end only was associated by a spark with the ball, but was at the same time related to the other end by a spark between the two.

1414. When the ball *c* was about an inch in diameter, the ends, *n* and *o*, about half an inch from it, and about 0.4 of an inch from each other, the two sparks to the ball could be obtained. When, for the purpose of bringing the sparks nearer together, the ends, *n* and *o* were brought closer to each other, then, unless very carefully adjusted, only one end had a spark with the ball, the other having a spark to it; and the least variation of position would cause either *n* or *o* to be the end which, giving the direct spark to the ball, was also the one through, or by means of which, the other discharged its electricity.

1415. On making the ball *c* smaller, I found that then it was needful to make the interval between the *n* and *o* larger in proportion to the distance between them and the ball *c*. On making *c* larger, I found I could diminish the interval, and so bring the two simultaneous separate sparks closer together, until, at last, the distance between them was not more at the widest part than 0.6 of their whole length.

1416. Numerous sparks were then passed and carefully observed. They were very rarely straight, but either curved or bent irregularly. In the average of cases they were I think, decidedly convex towards each other; perhaps two thirds presented more or less of this effect, the rest bulging more or less outwards. I was never able, however, to obtain sparks which, separately leaving the ends of the wires *n* and *o*, conjoined into one spark before they reached or communicated with the ball *c*. At present, therefore, though I think I saw a tendency in the sparks to unite, I cannot assert it as a fact.

1417. But there is one very interesting effect here analogous to, and it may be in part the same with, that I was searching for; I mean the increased facility of discharge where the spark passes. For instance, in the cases where one end, as *n*, discharged the electricity of both ends to the ball *c*, fig. 2., the electricity of the other end *o*, had to pass through an interval of air 1.5 times as great as that which it might have taken, by its direct passage between the end and the ball itself. In such cases, the eye could not distinguish, even by the use of WHEATSTONE'S means*, that the spark from the end *n* which contained both portions of the electricity, was a double spark. It could not have consisted of two sparks taking separate courses, for such an effect would have been visible to the eye; but it is just possible, that the spark of the first end *n* and its jar, passing at the smallest interval of time before that of the other *o*, had heated and expanded the air in

* Philosophical Transactions, 1834, pp. 584, 585.

its course, and made it so much more favorable to discharge, that the electricity of the end *o* preferred leading across to it and taking a very circuitous route, rather than the more direct one to the ball. It must, however, be remarked, in answer to this supposition, that the one spark between *d* and *e* would, by its influence, tend to produce simultaneous discharges at *n* and *o*, and certainly did so, when no preponderance was given to one wire over the other, as to the previous inductive effect (1414.).

1418. The fact, however, is, that disruptive discharge is favorable to itself. It is at the outset a case of tottering equilibrium: and if *time* be an element in discharge, in however minute a proportion (1436.), then the commencement of the act at any point favors its continuance and increase there, and portions of power will be discharged by a course which they would not otherwise have taken.

1419. The mere heat and expansion of the air itself by the the first portion of electricity which passes, must have a great influence in producing this result.

1420. As to the result itself, we see its influence in every spark that passes; for it is not the whole quantity which passes that determines the discharge, but merely that small portion of force which brings the deciding molecule (1370.) up to its maximum tension; then when its forces are subverted and discharge begins, all the rest passes by the same course, from the influence of the favoring circumstances just referred to; and whether it be the electricity on a square inch, or a thousand square inches of charged glass, the discharge is complete. Hereafter we shall find the influence of this effect in the formation of brushes (1435.); and it is not impossible that we may trace it producing the jagged spark and the forked lightning.

1421. The characters of the electric spark in *different gases* vary, and the variation *may* be due simply to the effect of the heat evolved at the moment. But it may also be due to that specific relation of the particles and the electric forces which I have assumed as the basis of a theory of induction; the facts do not oppose such a view; and in that view, the variation strengthens the argument for molecular action, as it would seem to shew the influence of the latter in every part of the electrical effect (1423, 1454).

1422. The appearances of the sparks in different gases have often been observed and recorded,* but I think it not

* See Van Marum's description of the Teylerian Machine, vol. i. p. 112. and vol. ii. p. 196; also Ency. Britan. vol. vi., Article Electricity, pp. 505, 507.

out of place to notice briefly the following results; they were obtained with balls of brass, (platina surfaces would have been better,) and at common pressures. In *air*, the sparks have that intense light and bluish colour which are so well known, and often have faint or dark parts in their course, when the quantity of electricity passing is not great. In *nitrogen*, they are very beautiful, having the same general appearance as in air, but have decidedly more colour of a bluish or purple character, and I thought were remarkably sonorous. In *oxygen*, the sparks were whiter than in air or nitrogen, and I think not so brilliant. In *hydrogen*, they had a very fine crimson colour, not due to its rarity, for the character passed away as the atmosphere was rarefied (1459).^{*} Very little sound was produced in this gas; but that is a consequence of its physical condition.[†] In *carbonic acid gas*, the colour was similar to that of the spark in air, but with a little green in it: the sparks were remarkably irregular in form, more so than in common air: they could also, under similar circumstances as to size of ball, &c. be obtained much longer than in air, the gas shewing a singular readiness to pass the discharge in the form of spark. In *muriatic acid gas*, the spark was nearly white: it was always bright throughout, never presenting those dark parts which happen in air, nitrogen, and some other gases. The gas was dry, and during the whole experiment the surface of the glass globe within remained quite dry and bright. In *coal gas*, the spark was sometimes green, sometimes red, and occasionally one part was green and another red. Black parts also occur very suddenly in the line of the spark, i. e. they are not connected by any dull part with bright portions, but the two seem to join directly one with the other.

1423. These varieties of character impress my mind with a feeling, that they are due to a direct relation of the electric powers to the particles of the dielectric through which the discharge occurs, and are not the mere results of a casual ignition or a secondary kind of action of the electricity, upon the particles which it finds in its course and thrusts aside in its passage (1454.).

1424. The spark may be obtained in media which are far denser than air, as in oil of turpentine, olive oil, resin, glass, &c.: it may also be obtained in bodies which being denser likewise approximate to the condition of conductors, as sper-

^{*} Van Marum, says they are about four times as large in hydrogen as in air, vol. i. p. 122.

[†] Leslie.

maceti, water, &c. But in these cases, nothing occurs which, as far as I can perceive, is at all hostile to the general views I have endeavored to advocate.

The electrical brush.

1425. The *brush* is the next form of disruptive discharge which I will consider. There are many ways of obtaining it, or rather of exalting its characters; and all these ways illustrate the principles upon which it is produced. If an insulated conductor, connected with the positive conductor of an electrical machine, have a metal rod 0·3 of an inch in diameter, projecting from it outwards from the machine, and terminating by a rounded end or a small ball, it will generally give good brushes; or, if the machine be not in good action, then many ways of assisting the formation of the brush can be resorted to; thus, the hand, or any *large* conducting surface may be approached towards the termination to increase inductive force (1374.): or the termination may be smaller and of badly conducting matter, as wood: or sparks may be taken between the prime conductor of the machine and the secondary conductor to which the termination giving brushes belongs: or, which gives to the brushes exceedingly fine characters and great magnitude, the air around the termination may be rarefied more or less, either by heat or the air pump; the former favorable circumstances being also continued.

1426. The brush when obtained by a powerful machine on a ball about 0·7 of an inch in diameter, at the end of a long brass rod attached to the positive prime conductor, had the general appearance as to form represented in fig. 3.: a short conical bright part or root appeared at the middle part of the ball projecting directly from it, which at a little distance from the ball, broke out suddenly into a wide brush of pale ramifications having a quivering motion, and being accompanied at the same time with a low dull chattering sound.

1427. At first the brush seems continuous, but Professor Wheatstone has shewn that the whole phenomenon consists of successive intermitting discharges.* If the eye be passed rapidly, not by a motion of the head, but of the eyeball itself, across the direction of the brush, by first looking steadfastly about 10° or 15° above, and then instantly as much below it, the general brush will be resolved into a number of individual brushes, standing in a row upon the line which the eye passed over; each elementary brush being the result of a single dis-

* Philosophical Transactions, 1834, p. 586.

charge, and the space between them representing both the time during which the eye was passing over that space, and that which elapsed between one discharge and another.

1428. The single brushes could easily be separated to eight or ten times their own width, but were not at the same time extended, i. e. they did not become more indefinite in shape, but, on the contrary, less so, each being more distinct in form, ramification, and character, because of its separation from the others, in its effects upon the eye. Each, therefore, was instantaneous in its existence (1436.). Each had the conical root complete (1426.).

1429. On using a smaller ball, the general brush was smaller, and the sound, though weaker, more continuous. On resolving the brush into its elementary parts, as before, these were found to occur at much shorter intervals than in the former case, but still the discharge was intermitting.

1430. Employing a wire with a round end, the brush was still smaller, but, as before, separable into successive discharges. The sound, though feebler, was higher in pitch, being a distinct musical note.

1431. The sound is, in fact, due to the recurrence of the noise of each separate discharge, and these, happening at intervals nearly equal under ordinary circumstances, cause a definite note to be heard, which rising in pitch with the increased rapidity and regularity of the intermitting discharges, gives a ready and accurate measure of the intervals, and so may be used in any case when the discharge is heard, even though the appearances may not be seen, to determine the element of *time*. So, also, when, by bringing the hand towards a projecting rod or ball, the pitch of the tone produced by a brushy discharge increases, the effect informs us that we have increased the induction (1374.), and by that means increased the rapidity of the alternations of charge and discharge.

1432. By using wires with finer terminations, smaller brushes were obtained, until they could hardly be distinguished as brushes; but as long as *sound* was heard, the discharge could be ascertained by the eye to be intermitting; and when the sound ceased, the light became *continuous* as a glow (1359. 1405.).

1433. To those not accustomed to use the eye in the manner I have described, or, in cases where the recurrence is too quick for any unassisted eye, the beautiful revolving mirror of Professor Wheatstone* will be useful for such developments

* Philosophical Transactions, 1834, pp. 584, 585.

of condition as those mentioned above. Another excellent process is to produce the brush or other luminous phenomenon on the end of a rod held in the hand opposite to a charged positive or negative conductor, and then move the rod rapidly from side to side whilst the eye remains still. The successive discharges occur of course in different places, and the state of things before, at, and after a single coruscation or brush can be exceeding well separated.

1434. The *brush* is in reality a discharge between a bad or a non-conductor and either a conductor or another non-conductor. Under common circumstances, the brush is a discharge between a conductor and air, and I conceive it to take place in some thing like the following manner. When the end of an electrified rod projects into the middle of a room induction takes place between it and the walls of the room, across the dielectric, air; and the lines of inductive force accumulate upon the end in greater quantity than elsewhere, or the particles of air at the end of the rod are more highly polarized than those at any other part of the rod, for the reasons already given (1374.). The particles of air situated in sections across these lines of force are least polarized in sections towards the walls, and most polarized in those nearer to the end of the wires (1369.): thus, it may well happen, that a particle at the end of the wire is at a tension that will immediately terminate in discharge, whilst in those even only a few inches off, the tension is still beneath that point. But suppose the rod to be charged positively, a particle of air A, fig. 4. next it, being polarized, and having of course its negative force directed towards the rod and its positive force outwards; the instant that discharge takes place between the positive force of the particle of the rod opposite the air and the negative force of the particle of air towards the rod, the whole particle of air becomes positively electrified; and when, the next instant, the discharged part of the rod resumes its positive state, by conduction from the surface of metal behind, it not only acts on the particles beyond A, by throwing A into a polarized state again, but A itself, because of its charged state, exerts a distinct inductive act towards these further particles, and the tension is consequently so much exalted between A and B, that discharge takes place there also, as well as again between the metal and A.

1435. In addition to this effect, it has been shewn, that, the act of discharge having once commenced, the whole operation, like a case of unstable equilibrium, is hastened to a conclusion (1370. 1418.), the rest of the act being facilitated in its occurrence, and other electricity than that which caused

the first necessary tension hurrying to the spot. When, therefore, disruptive discharge has once commenced at the root of a brush, the electric force which has been accumulating in the conductor attached to the rod, finds a more ready discharge there than elsewhere, and will at once follow the course marked out as it were for it, thus leaving the conductor in a partially discharged state, and the air about the end of the wire in a charged condition; and the time necessary for restoring the full charge of the conductor, and the dispersion of the charged air in a greater or smaller degree, by the joint forces of repulsion from the conductor and attraction towards the walls of the room, to which its inductive action is directed is just that time which forms the interval between brush and brush (1420. 1427. 1431.)

1436. The words of this description are long, but there is nothing in the act or the forces on which it depends to prevent its being *instantaneous*, as far as we can estimate and measure it. The consideration of *time* is, however, important in several points of view (1418.), and in reference to disruptive discharge, it seemed from theory far more probable that it might be detected in a brush than in a spark, for in a brush, the particles in the line through which the discharge passes are in very different states as to intensity, and the discharge is already complete in its act at the root of the brush, before the particles at the extremity of the ramifications have yet attained their maximum intensity.

1437. I consider brush discharge as, probably, a successive effect in this way. Discharge begins at the root (1426.), and, extending itself in succession to all parts of the single brush, continues to go on at the root and the previously formed parts until the whole brush is complete; then, by the fall in intensity and power at the conductor, it ceases at once in all parts, to be renewed, when that power has risen again to a sufficient degree. But in a spark, the particles in the line of discharge being, from the circumstances, nearly alike in their intensity of polarization, suffer discharge so nearly at the same moment as to make the time quite insensible to us.

1438. Mr. Wheatstone has already made experiments which fully illustrate this point. He found that the brush generally had a sensible duration, but that with his highest capabilities he could not detect any such effect in the spark.* I repeated his experiment on the the brush, though with more imperfect means, to ascertain whether I could distinguish a longer duration in the stem or root of the brush than in the ex-

* Philosophical Transactions, 1836, pp. 586, 590.

tremities, and the appearances were such as to make me think an effect of this kind was produced.

1439. That the discharge breaks into several ramifications, and by them passes through portions of air alike, or nearly alike, as to polarization and the degree of tension the particles there have acquired, is a very natural result of the previous state of things, and sooner to be expected than that the discharge should continue to go straight out into space in a single line amongst those particles which, being at a distance from the end of the rod, are in a lower state of tension than those which are near : and whilst we cannot but conclude, that those parts where the branches of a single brush appear, are more favorably circumstanced for discharge than the darker parts between the ramifications, we may also conclude, that in those parts where the light of concomitant discharge is equal, there the circumstances are nearly equal also. The single brushes are by no means of the same particular shape even when they are observed without displacement of the rod or surrounding objects (1427. 1433.), and the successive discharges may be considered as taking place into the mass of air around, through different roads at each brush, according as minute circumstances, as dust, &c. (1391. 1392.) may have favored the course by one set of particles rather than another.

1440. Brush discharge does not essentially require any current of the medium in which the brush appears : the current almost always occurs, but is a consequence of the brush, and will be considered hereafter. On holding a blunt point positively charged towards uninsulated water, a star or glow appeared on the point, a current of air passed from it, and the surface of the water was depressed ; but on bringing the point so near that sonorous brushes passed, then the current of air instantly ceased, and the surface of the water became level.

1441. The discharge by a brush is not to all the particles of air that are near the electrified conductor from which the brush issues ; only those parts where the ramifications pass are electrified : the air in the central dark parts between them receives no charge, and, in fact, at the time of discharge, has its electric and inductive tension considerably lowered. For consider fig. 14. to represent a single positive brush ;—the induction before the discharge is from the end of the rod outwards, in diverging lines towards the distant conductors, as the walls of the room, &c., and a particle at *a* has polarity of a certain degree of tension, and tends with a certain force to become charged ; but at the moment of discharge, the air in

the ramifications *b* and *d*, acquiring also a positive state, opposes its influence to that of the positive conductor on *a*, and the tension of the particle at *a* is therefore diminished rather than increased. The charged particles at *b* and *d* are now inductive bodies, but their lines of inductive action are still outwards towards the walls of the room; the direction of the polarity and the tendency of other particles to charge from these, being governed by, or in conformity with, these lines of force.

1442. The particles that are charged are probably very highly charged, but, the medium being a non-conductor, they cannot communicate that state to their neighbours. They travel, therefore, under the influence of the repulsive and attractive forces, from the charged conductor towards the nearest uninsulated conductor, or the nearest body in a different state to themselves, just as charged particles of dust would travel, and are then discharged; each particle acting, in its course, as a centre of inductive force upon any bodies near which it may come.

1443. The travelling of these charged particles when they are numerous, causes wind and currents, but these will come into consideration under *carrying discharge* (1319.). When air is said to be electrified, and it frequently assumes this state near electrical machines, it consists, according to my view, of a mixture of electrified and un-electrified particles, the latter being in very large proportion to the former. When we gather electricity from air by a flame or by wires, it is either by the actual discharge of these particles, or by effects dependent on their inductive action, a case of either kind being produceable a pleasure. That the law of equality between the two forces or forms of force in inductive action is as strictly preserved in these as in other cases, is fully shewn by the fact, formerly stated (1173. 1174.), that, however strongly air in a vessel might be charged positively, there was an exactly equal amount of negative force on the inner surface of the vessel itself, for no residual portion of either the one or the other electricity could be obtained.

1444. I have nowhere said, nor does it follow, that the air is charged only where the luminous brush appears. The charging may extend beyond those parts which are visible, i. e. particles to the right or left of the lines of light may receive electricity, the parts which are luminous being so only because much electricity is passing by them to other parts (1437.); just as in a spark discharge the light is greater as more electricity passes, though it has no necessary relation to the quantity required to commence discharge (1370. 1420.).

Hence the form we see in a brush may by no means represent the whole quantity of air electrified; for an invisible portion, clothing the visible form to a certain depth, may, at the same time, receive its charge.

1445. Several effects which I have met with in muriatic acid gas tend to make me believe, that gaseous body allows of a dark discharge. At the same time, it is quite clear from theory, that in some gases, the reverse of this may occur, i. e. that the charging of the air may not extend even so far as the light. We do not know as yet enough of the electric light to be able to state on what it depends, and it is very possible that, when electricity bursts forth into air, all the particles of which are in a state of tension, light may be evolved by such as, being very near to, are not of, those which actually receive a charge at the time.

1446. The further a brush extends in a gas, the further no doubt is the charge or discharge carried forward; but this may vary between different gases, and yet the intensity required for the first moment of discharge not vary in the same, but in some other proportion. Thus with respect to nitrogen and muriatic acid gases, the former, as far as my experiments have proceeded, produces far finer and larger brushes than the latter (1458. 1462.), but the intensity required to commence discharge is much higher for the latter than the former (1395.). Here again, therefore, as in many other qualities, specific differences are presented by different gaseous dielectrics, and so prove the special relation of the latter to the act and the phenomena of induction.

1447. To sum up these considerations respecting the character and condition of the brush, I may state that it is a spark to air; a diffusion of electric force to matter, not by conduction, but disruptive discharge; a dilute spark which, passing to very badly conducting matter, frequently discharges but a small portion of the power stored up in the conductor; for as the air charged reacts on the conductor, whilst the conductor, by loss of electricity, sinks in its force, the discharge quickly ceases, until by the dispersion of the charged air and the renewal of the excited conditions of the conductor, circumstances have risen up to their first effective condition, again to cause discharge, and again to fall and rise.

1448. The brush and spark gradually pass into one another. Making a small ball positive by a good electrical machine with a large prime conductor, and approaching a large uninsulated discharging ball towards it, very beautiful variations from the spark to the brush may be obtained. The drawings of long and powerful sparks, given by

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Van Marum,* Harris,† and others, also indicate the same phenomena. As far as I have observed, whenever the spark has been brushy in air of common pressures the whole of the electricity has not been discharged, but only portions of it, more or less according to circumstances: whereas, whenever the effect has been a distinct spark throughout the whole of its course, the discharge has been perfect, provided no interruption had been made to it elsewhere, in the discharging circuit, than where the spark occurred.

1449. When an electrical brush from an inch to six inches in length or more is issuing into free air, it has the form given, fig. 3. But if the hand, a ball, or any knobbed conductor be brought near, the extremities of the coruscations turn towards it and each other, and the whole assumes various forms according to circumstances, as in figs. 5, 6, and 7. The influence of the circumstances in each case is easily traced, and I might describe it here, but that I should be ashamed to occupy the time of the Society in things so evident. But how beautifully does the curvature of the ramifications illustrate the curved form of the lines of inductive force existing previous to the discharge! for the former are consequences of the latter, and take their course, in each discharge, where the previous inductive tension had been raised to the proper degree. They represent these curves just as well as iron filings represent magnetic curves, the visible effects in both cases being the consequences of the action of the forces in *the places where* the effects appear. The phenomena, therefore, constitute additional and powerful testimony (1216. 1230.) to that already given in favor both of induction through dielectrics in curved lines (1231.), and of the lateral relation of these lines, by an effect equivalent to a repulsion producing divergence, or, as in the cases figured, the bulging form.

1450. In reference to the theory of molecular inductive action, I may also add here, the proof deducible from the long brushy ramifying spark which may be obtained between a small ball on the positive conductor of an electrical machine, and a larger one at a distance (1448.). What a fine illustration that spark affords of the previous condition of *all* the particles of the dielectric between the surfaces of discharge, and how unlike the appearances are to any which would be deduced from the theory which assumes inductive action to be

* Description of the Teylerian machine, vol. i. pp. 28. 32.; vol. ii. p. 226. &c.
† Philosophical Transactions, 1834. p. 243.

action at a distance, in straight lines only; and charge, as being electricity retained upon the surface of conductors by the mere pressure of the atmosphere!

1451. When the brush is obtained in rarefied air, the appearances vary greatly, according to circumstances, and are exceedingly beautiful. Sometimes a brush may be formed of only six or seven branches, these being broad and highly luminous, of a purple colour, and in some parts an inch or more apart:—by a spark discharge at the prime conductor (1455.) single brushes may be obtained at pleasure. Discharge in the form of a brush is favored by rarefaction of the air, in the same manner and for the same reason as discharge in the form of a spark (1375.); but in every case there is previous induction and charge through the dielectric, and polarity of its particles (1437.), the induction being, as in any other instance, alternately raised by the machine and lowered by the discharge. In certain experiments the rarefaction was increased to the utmost degree, and the opposed conducting surfaces brought as near together as possible without producing the glow: the brushes then contracted in their lateral dimensions, and recurred so rapidly as to form an apparently continuous arc of light from metal to metal. Still the discharge could be observed to intermit (1427.), so that even under these high conditions, induction preceded each single brush, and the tense polarized condition of the contiguous particles was a necessary preparation for the discharge itself.

1452. The brush form of disruptive discharge may be obtained not only in air and gases, but also in much denser media. I procured it in oil of turpentine from the end of a wire going through a glass tube into the fluid contained in a metal vessel. The brush was small and very difficult to obtain; the ramifications were simple, and stretched out from each other diverging very much. The light was exceedingly feeble, a perfectly dark room being required for its observation. When a few solid particles, as of dust or silk, were in the liquid, the brush was produced with much greater facility.

1453. The running together or coalescence of different lines of discharge (1412.) is very beautifully shewn in the brush in air. This point may present a little difficulty to those who are not accustomed to see in every discharge an equal exertion of power in opposite directions, a positive brush being considered by such (perhaps in consequence of the common phrase *direction of a current*) as indicating a breaking forth in different directions of the original force, rather than a

tendency to convergence and union in one line of passage. But the ordinary case of the brush may be compared, for its illustration, with that in which, by holding the knuckle opposite to highly excited glass, a discharge occurs, the ramifications of a brush then leading from the glass and converging into a spark on the knuckle. Though a difficult experiment to make, it is possible to obtain discharge between highly excited shell-lac and the excited glass of a machine: when the discharge passes, it is, from the nature of the charged bodies, brush at each end and spark in the middle, beautifully illustrating that tendency of discharge to facilitate like action, which, I have described in a former page (1418.).

1454. The brush has *specific characters* in different gases, indicating a relation to the particles of these bodies even in a stronger degree than the spark (1422. 1423.). This effect is in strong contrast with the non-variation caused by the use of different substances as *conductors* from which the brushes are to originate. Thus, using such bodies as wood, card, charcoal, nitre, citric acid, oxalic acid, oxide of lead, chloride of lead, carbonate of potassa, potassa fuso, strong solution of potash, oil of vitriol, sulphur, sulphuret of antimony, and hæmatite, no variation in the character of the brushes was obtained, except that (dependent upon their effect as better or worse conductors) of causing discharge with more or less readiness and quickness from the machine.*

1455. The following are a few of the effects I observed in different gases at the positively charged surfaces, and with atmospheres varying in their pressure. The general effect of rarefaction was the same for all the gases: at first, sparks passed; these gradually were converted into brushes, which became larger and more distinct in their ramifications, until, upon further rarefaction, the latter began to collapse and draw in upon each other, till they formed a stream across from conductor to conductor: then a few lateral streams shot out towards the glass of the vessel from the conductors; these became thick, flossy, and soft in appearance, and were succeeded by the full constant glow which covered the discharging wire. The phenomena varied with the size of the vessel (1477.), the degree of rarefaction, and the discharge of electricity from the machine. When the latter was in successive sparks, they were most beautiful, the effect of a spark from a small machine being equal to, and often surpass-

* Exception must, of course, be made of those cases where the root of the brush, becoming a spark, causes a little diffusion or even decomposition of the matter there, and so gains more or less of a particular colour at that part.

ing, that produced by the *constant* discharge of a far more powerful one.

1456. *Air*.—Fine positive brushes are easily obtained in air at common pressures, and possess the well-known purplish light. When the air is rarefied, the ramifications are very long, filling the globe (1477.), the light is greatly increased, and is of a beautiful purple colour, with an occasional rose tint in it.

1457. *Oxygen*.—At common pressures, the brush is very close and compressed, and of a dull whitish colour somewhat purplish, but all the characters very poor compared to those in air.

1458. *Nitrogen* gives brushes with great facility at the positive surface, far beyond any other gas I have tried: they are almost always fine in form, light, and colour, and in rarefied nitrogen are magnificent. They surpass the discharges in any other gas as to the quantity of light evolved.

1459. *Hydrogen*, at common pressures, gave a better brush than oxygen, but did not equal nitrogen; the colour was greenish grey. In rarefied hydrogen, the ramifications were very fine in form and distinctness, but pale in colour, with a soft and velvety appearance, and not at all equal to those in nitrogen. In the rarest state of the gas, the colour of the light was a pale grey green.

1460. *Coal gas*.—The brushes were rather difficult to produce, the contrast with nitrogen being great in this respect. They were short and strong, generally of a greenish colour, and possessing much of the spark character: for, occurring on both the positive and negative terminations, often when there was a dark interval of some length between the two brushes, still the quick, sharp sound of the spark was produced, as if the discharge had been sudden through this gas, and partaking, in that respect, of the character of a spark. In rare coal gas, the forms were better, but the light very poor and the colour grey.

1461. *Carbonic acid gas* produces a very poor brush at common pressures, as regards either size, light, or colour; and this is probably connected with the tendency which this gas has to discharge the electricity as a spark (1422.). In rarefied carbonic acid, the brush is better in form, but weak as to light, being of a dull greenish or purplish hue, varying with the pressure and other circumstances.

1462. *Muriatic acid gas*.—It is very difficult to obtain the brush in this gas at common pressures. On gradually increasing the distance of the rounded ends, the sparks suddenly ceased when the interval was about an inch, and the

discharge, which was still through the gas in the globe, was silent and dark. Occasionally a very short brush could for a few moments be obtained, but it quickly disappeared again. Even when the intermitting spark current (1455.) from the machine was used, still I could only with difficulty obtain a brush, and that very short, though I used rods with rounded terminations (about 0.25 of an inch in diameter) which had before given them most freely in air and nitrogen. During the time of this difficulty with the muriatic gas, magnificent brushes were passing off from different parts of the machine into the surrounding air. On rarefying the gas, the formation of the brush was facilitated, but it was generally of a low squat form, very poor in light, and very similar on both the positive and negative surfaces. On rarefying the gas still more, a few large ramifications were obtained of a pale bluish colour, utterly unlike those in nitrogen.

1463. In all the gases, the different forms of disruptive discharge may be linked together and gradually traced from one extreme to the other, i. e. from the spark to the glow (1405.), or, it may be, to a still further condition to be called dark discharge; but it is, nevertheless, very surprising to see what a specific character each keeps whilst under the predominance of the general law. Thus, in muriatic acid, the brush is very difficult to obtain, and there comes in its place almost a dark discharge, partaking of the readiness of the spark action. Moreover, in muriatic acid, I have *never* observed the spark with any dark interval in it. In nitrogen, the spark readily changes its character into that of brush. In carbonic acid gas, there seems to be a facility to occasion spark discharge, whilst yet that gas is unlike nitrogen in the facility of the latter to form brushes, and unlike muriatic acid in its own facility to continue the spark. These differences add further force, first to the observations already made respecting the spark in various gases (1422. 1423.), and then, to the proofs deducible from it, of the relation of the electrical forces to the particles of matter.

1464. The peculiar characters of nitrogen in relation to the electric discharge (1422. 1458.) must, evidently, have an important influence over the form and even the occurrence of lightning. Being that gas which most readily produces coruscations, and, by them, extends discharge to a greater distance than any other gas tried, it is also that which constitutes four fifths of our atmosphere; and as, in atmospheric electrical phenomena, one, and sometimes both the inductive forces are resident on the particles of the air, which, though probably affected as to conducting power by the aqueous

particles in it, cannot be considered as a good conductor so, the peculiar power possessed by nitrogen, to originate and effect discharge in the form of a brush or of ramifications, has, probably, an important relation to its electrical service in nature, as it most seriously affects the character and condition of the discharge when made. The whole subject of discharge from and through gases is a most important one to science, and, if only in reference to atmospheric electricity, deserves extensive and close experimental investigation.

Difference of discharge at the positive and negative conducting surfaces.

1465. I have avoided speaking of this well-known phenomenon more than was quite necessary, that I might bring together here what I have to say on the subject. When the brush discharge is observed in air at the positive and negative surfaces, there is a very striking difference, the true and full comprehension of which would, no doubt, be of the utmost importance to the physics of electricity; it would throw great light on our present subject, i. e. the molecular action of dielectrics under induction, and its consequences, and seems very open to, and accessible by, experimental inquiry.

1466. The difference in question used to be expressed in former times by saying, that a point charged positively gave brushes into the air, whilst the same point charged negatively gave a star. This is true only of bad conductors, or of metallic conductors charged intermittingly, or otherwise controlled by collateral induction. If metallic points project *freely* into the air, the positive and negative light upon them differ very little in appearance, and the difference can be observed only upon close examination.

1467. The effect varies exceedingly under different circumstances, but, as we must set out from some position, may perhaps be stated thus: if a metallic wire with a rounded termination in free air be used to produce the brushy discharge, then the brushes obtained when the wire is charged negatively are very poor and small, by comparison with those produced when the charge is positive. Or if a large metal ball connected with the electrical machine be charged *positively*, and a fine uninsulated point be gradually brought towards it, a star appears on the point when at a considerable distance, which though it becomes brighter, does not change its form of a star until it is close up to the ball: whereas, if the ball be charged negatively, the point at a considerable distance has a star on it as before; but when brought nearer,

(in my case to the distance of $1\frac{1}{2}$ inch,) a brush formed on it, extending to the negative ball; and when still nearer, (at $\frac{1}{2}$ of an inch distance,) the brush ceased, and bright sparks passed. These variations, I believe, include the whole series of differences, and they seem to shew at once, that the negative surface tends to retain its discharging character unchanged, whilst the positive surface, under similar circumstances, permits of great variation.

1468. There are several points in the character of the negative discharge to air which it is important to observe. A metal rod, 0.3 of an inch in diameter, with a rounded end projecting into the air, was charged negatively, and gave a short noisy brush (fig. 8.). It was ascertained both by sight (1427. 1433.) and sound (1431.), that the successive discharges were very rapid in their recurrence, being seven or eight times more numerous in the same period, than those produced when the rod was charged positively to an equal degree. When the rod was positive, it was easy, by working the machine a little quicker, to replace the brush by a glow (1405. 1463.), but when it was negative no efforts could produce this change. Even by bringing the hand opposite the wire, the only effect was to increase the number of brush discharges in a given period, raising at the same time the sound to a higher pitch.

1469. A point opposite the negative brush exhibited a star, and as it was approximated caused the size and sound of the negative brush to diminish. and, at last, to cease, leaving the negative end silent and dark, yet effective as to discharge.

1470. When the round end of a smaller wire (fig. 9.) was advanced towards the negative brush, it (becoming positive by induction) exhibited the quiet glow at 8 inches distance, the negative brush continuing. When nearer, the pitch of the sound of the negative brush rose, indicating quicker intermittences (1431.); still nearer, the positive end threw off ramifications and distinct brushes; at the same time, the negative brush contracted in its lateral directions and collected together, giving a peculiar narrow longish brush, in shape like a hair pencil, the two brushes existing at once, but very different in their form and appearance, and especially in the more rapid recurrence of the negative discharges than of the positive. On using a smaller positive wire for the same experiment, the glow first appeared on it, and then the brush, the negative brush being affected at the same time; and the two at one distance became exceedingly alike in appearance, and the sounds, I thought, were in unison; at all events they were in harmony, so that the intermissions of discharge were

either isochronous, or a simple ratio existed between the intervals. With a higher action of the machine, the wires being retained unaltered, the negative surface would become dark and silent, and a glow appear on the positive one. A still higher action changed the latter into a spark. Finer positive wires gave other variations of these effects, which I must not allow myself to go into here.

1471. A thinner rod was now connected with the negative conductor in place of the larger one (1468.), its termination being gradually diminished to a blunt point, as in fig. 10, ; and it was beautiful to observe that, notwithstanding the variation of the brush, the same general order of effects was produced. The end gave a small sonorous negative brush, which the approach of the hand or of a large conducting surface did not alter, until it was so near as to produce a spark. A fine point opposite to it was luminous at a distance ; being nearer it did not destroy the light and sound of the negative brush, but only tended to have a brush produced on itself, which, at a still nearer distance, passed into a spark joining the two surfaces.

1472. When the distinct negative and positive brushes are produced simultaneously in relation to each other in air, the former almost always has a contracted form, as in fig. 11., very much indeed resembling the figure which the positive brush itself has when influenced by the lateral vicinity of positive parts acting by induction. Thus a brush issuing from a point in the re-entering angle of a positive conductor has the same compressed form (fig. 12.).

1473. The character of the negative brush is not affected by the chemical nature of the substances of the conductors (1454.), but only by their possession of the conducting power in a greater or smaller degree.

1474. Rarefaction of common air about a negative ball or blunt point facilitated the development of the negative brush, the effect being, I think, greater than on a positive brush, though great on both. Extensive ramifications could be obtained from a ball or end electrified negatively to the plate of the air-pump on which the jar containing it stood.

1475. A very important variation of the relative forms and conditions of the positive and negative brush takes place on varying the dielectric in which they are produced. The difference is so very great that it points to a specific relation of this form of discharge to the particular gas in which it takes place, and opposes the idea that gases are but obstructions to the discharge, acting one like another and merely in proportion to their pressure (1377).

1476. In *air*, the superiority of the positive brush is well known (1467. 1472.). In *nitrogen*, it is as great or even greater than in *air* (1458.). In *hydrogen*, the positive brush loses a part of its superiority, not being so good as in *nitrogen* or *air*; whilst the negative brush does not seem injured (1459.). In *oxygen*, the positive brush is compressed and poor (1457.); whilst the negative did not sink in character: the two were so alike that the eye frequently could not tell the one from the other, and this similarity continued when the oxygen was gradually rarefied. In *coal gas*, the brushes are difficult of production as compared to *nitrogen* (1460.), and the positive not much superior to the negative in its character, either at common or low pressures. In *carbonic acid gas*, this approximation of character also occurred. In *muriatic acid gas* the positive brush was very little better than the negative, and both difficult to produce (1462.) as compared with the facility in *nitrogen* or *air*.

1477. These experiments were made with rods of brass about a quarter of an inch thick having rounded ends, the ends being opposed in a glass globe 7 inches in diameter, containing the gas to be experimented with. The electric machine was used to communicate directly, sometimes the positive, and sometimes the negative, state, to the rod in connexion with it.

1478. Thus we see that, notwithstanding there is a general difference in favor of the superiority of the positive brush over the negative, that difference is at its maximum in *nitrogen* and *air*; whilst in *carbonic acid*, *muriatic acid*, *coal gas*, and *oxygen* it diminishes, and at last becomes almost nothing. So that in this particular effect, as in all others yet examined, the evidence is in favor of that view which refers the results to a direct relation of the electric forces with the molecules of the matter concerned in the action (1421. 1423. 1463.). Even when special phenomena arise under the operation of the general law, the theory adopted seems fully competent to meet the case.

1479. Before I proceed further in tracing the probable cause of the difference between the positive and negative brush discharge, I wish to know the results of a few experiments which are in course of preparation: and thinking this Series of Researches long enough, I shall here close it with the expectation of being able in a few weeks to renew the inquiry, and entirely redeem my pledge (1306.).

Royal Institution, Dec. 23rd, 1837.

XXIV.—On *Electro-Magnetic Forces*. By J. P. JOULE, Esq.

29. In resuming the relation of my researches, I shall dismiss for the present the investigation of electro-magnetic forces as applied to the movement of machines, and consider the laws which govern that peculiar condition which is assumed on the completion of the ferruginous circuit—the lifting or sustaining power of the electro-magnet.

30. Although this wonderful property is known to all, and a variety of forms has been given to the electro-magnet both as regards the bulk and shape of its iron, and the length and number of its magnetizing spirals, I am not aware that any general rules have been laid down for its manufacture, which is a circumstance the more to be regretted, as it has led some to imagine that the different capabilities of various arrangements, are the consequence of causes too many and too recondite to be unravelled. I shall attempt in this paper to throw some light upon this subject, and shall describe a construction attended by far greater results than have hitherto been produced.—It was my desire to make my experiments as exact as possible, and as I wish the relation of them to be clear and definite, I shall begin with some observations on the *measure* of current electricity indicated by the galvanometer, an instrument not only useful but absolutely essential in an inquiry of this nature.

31. The great difficulty, if not the absolute impossibility, of understanding experiments such as these and comparing them with one another, arises in general from incomplete descriptions of apparatus, and in particular from the arbitrary and vague numbers which are used in characterizing electric currents. Such a practice might be tolerated in the infancy of the science, but in its present state of advancement greater precision and propriety are imperatively demanded. I have therefore determined for my own part to abandon my old quantity numbers and to express my results on the basis of an *unit* which shall be at once scientific and convenient.

32. That proposed by Dr. Faraday is, I believe, the only standard of this kind that has been suggested. His discovery of the definite quantity of electricity associated with the atoms or chemical equivalents of bodies, has induced him to use the *voltameter* as a measurer, and to propose that the hundredth part of a cubic inch of the mixed gases should constitute the *degree*.* There can be no doubt that this system offers superior advantages to the experimenter in some circumstances,

* *Experimental Researches*, Series vii. (736).

and when the above instrument is employed. However, as I am not aware that it has been used in the researches of any electrician, not excepting those of Faraday himself, I have not hesitated to advance what I think to be more appropriate as well as more generally advantageous. It is thus simply stated.

33. 1. *A degree of static electricity is that quantity which is just able to decompose nine grains of water.* 2. *A degree of current electricity is the same amount propagated during each hour of time;* and 3. *Where both time and length of conductor are elements, as in electro-dynamics, a degree of electric force, or of electro-momentum, is indicated by that same quantity (a degree of static electricity,) propagated through the space of one foot in one hour of time.* Whenever in future I speak of *degrees*, I shall intend those which I have just defined.

34. As 9 is the atomic weight of water it is obvious how greatly my *degree* will facilitate the calculation of electro-chemical decompositions. I may in this place adduce an illustration from electro-type engraving: here, if a galvanometer graduated according to my scale were included in the circuit, it would only be necessary to multiply the degrees (33,2) of its indication by 32, the equivalent of copper, and this again by the time in hours during which the work has been carried on, to obtain the weight of copper in grains which has been precipitated, and there would therefore be no occasion whatever to disturb the arrangement until calculation had shewn that the proper quantity of copper was cast. For instance, in an experiment of my own, I caused two electrodes of copper to terminate in a solution of the sulphate slightly acidulated by sulphuric acid. The negative electrode, upon which of course the copper was deposited, consisted of a disc an inch and a half in diameter; the positive, of a small coil of wire. A current of the mean quantity°.415 was then passed through the apparatus for one hour and a quarter; hence, according to the rule, $.415 \times 32 \times 1.25 = 16.6$ gr. the weight of copper which should *theoretically* be deposited. The *real* quantity, well washed and dried, was 15.6 gr. The deficiency of one grain was the evident consequence of the consumption of a part of the electricity in the decomposition of *water*, which was plainly indicated by a slight evolution of hydrogen at the negative pole.

35. The galvanometer of which I made use in the last series of experiments* (14,) was connected with an apparatus fur-

* "Annals," April, 1840. p. 476.

nished with very fine platinum wires. Voltaic currents of a large variety of intensities were then conducted through both instruments at once, and at the end of one, two, or three minutes the circuit was broken, and the hydrogen measured in a graduated glass tube. The mean of ten trials, none of which differed materially from the rest, added to half its bulk of oxygen, then corrected for temperature, barometrical pressure and force of vapour, and reduced to weight, gave .76 gr. of water decomposed in one hour by electricity indicated by each unit of my former quantity numbers; hence 11.8 of these last is equal to one *degree* (33,2) of my present scale.—The dimensions of the single coil of the above galvanometer are 12 inches by 6, and the deviation of its needle for one *degree* (33,2) is 34° of the graduated card. From these data it is easy to calculate with considerable accuracy the value of the indications of any similar instrument, bearing in mind that the electro-dynamic force produced by a constant quantity of electricity is directly as the number of coils and inversely as their linear dimensions.

36. The quantities of electricity which were brought into play in the subsequent experiments, were frequently so great that the needle of my galvanometer (14) was brought to an almost rectangular position when subject to their influence. I have, therefore, devised a new measurer, which I flatter myself will prove of greater service in some cases than the arrangement proposed for the same purpose by Mr. Iremonger.* Fig. 1, plate 4, is the plan of my instrument: *c, c*, is a rod of copper bent and fastened firmly to a strong wooden frame: *m*, is a magnetized cylindrical bar of steel, one foot long, and half an inch in diameter, supported slightly above the centre of gravity, (like the ordinary balance beam,) by knife-edges resting on hard concave surfaces of steel; a scale *s*, is attached to the nearer end of the magnet, for the purpose of receiving the weights by which the intensity of electricity is measured. Lastly, *r, r*, is a rest, the under surface of which, the magnet just touches when at zero.

37. In using this apparatus, it is merely necessary to adjust the magnet to zero, either by means of screws, weights, or (perhaps the most convenient in practice) by the attraction or repulsion of a steel magnet kept for the purpose. Then, on making the necessary battery communications at *c, c*, the scale *s* will rise with a force estimated by the weight, in grains, tenths, &c., which is required to reduce it again to zero. In my instrument, I have found that one degree (33,2) is indicated by .69 gr.

* "Annals," vol. iii. pp. 413, 414.

38. The value of this new galvanometer, (the sensibility of which may be increased at pleasure by multiplying the number of coils,) besides its usefulness in measuring copious currents, consists chiefly in its perfect independence of the terrestrial, as well as any other ordinary magnetic influence. In every possible situation, provided that the intensity of the balance bar is constant, and that no interference is induced *after* the adjustment to zero, the transmitted current is exactly proportional to the weight lifted by the scale, and I should have as much confidence in working with it on an iron steam-boat as if every particle of iron were removed entirely away.

39. I proceed now to describe my electro-magnets, which I had occasion to construct of very different sizes in order to develop any curious circumstance which might present itself.—A piece of cylindrical wrought iron, eight inches long, had a hole one inch in diameter, bored the whole length of its axis; one side was then planed until the hole was exposed sufficiently to separate the “poles” $\frac{1}{2}$ of an inch. Another piece of iron, also eight inches long, was then planed, and being secured with its face in contact with the other planed surface, the whole was turned into a cylinder eight inches long, three inches and three quarters in exterior, and one inch in interior, diameter. The larger piece was then covered with calico and wound with four copper wires (covered with silk) each 23 feet long and 1-11th of an inch in diameter, a quantity which was just sufficient to hide the exterior surface and entirely to fill the inside hole. I shall perhaps be better understood on reference to plate 4, fig. 2, where *m*, is the “horse shoe” on which I have drawn some lines to illustrate the position of the conducting wire, *a*, is the armature and *s. s. &c.*, are screws with eye holes for the purpose of suspension. This electro-magnet is designated No. 1, and the rest are numbered in the order of their description.

40. The iron which I used in the construction of a second was round, and 2·7 in. long, and half an inch in diameter. It was bent into an almost semicircular shape, and covered with 7 feet of well insulated copper wire 1-20th of an inch thick. The poles were half an inch asunder, and the wire completely filled the space between them.

41. A third electro-magnet was made of a piece of iron, .7 of an inch long, .37 in. broad, and .15 of an inch thick.—Its edges were reduced to such an extent that its transverse section was a perfect ellipsis. This also was bent into a semicircular shape; and was covered with 19 inches of silked copper wire, one-fortieth of an inch in diameter.

42. Anxious to procure a still larger variety, I made what might, from its extreme minuteness, be termed an *elementary electro-magnet*. It was the smallest I believe ever constructed; and consisted of a piece of good iron wire, one quarter of an inch long and 1-25th of an inch in diameter. It was bent into a semicircle, and was covered by three turns of *uninsulated* copper wire 1-40th of an inch in diameter.

43. The system of *levers* which was used in part of the subsequent experiments was found to be so convenient that I am induced to describe it in this place, although it may not involve any thing essentially new.—In fig. 2. *b, b, b, b,* are beams of ash, three inches square and ten feet long, strengthened by strong iron plating. These are fastened together in pairs, by boards nailed to their upper sides. *i, i,* are moveable iron bearings, and *f,* is the fulcrum, also moveable, and armed with iron; *w, w,* are strong pieces of wood which bear upon the levers and carry the hooks which are affixed to the electro-magnet No. 1, and its armature.—I subjoin some of the results obtained by this apparatus. The first column contains degrees of current electricity (33,2). The second gives the products of the numbers in the first column and the length in feet of wire wrapped round the magnet; it contains therefore, degrees of electric force (33,3). Lastly, the fourth expresses the weight carried in pounds avordupois.

TABLE I.

Electro-magnet, No. 1. (39)—Weight of iron, 15lbs; length of wire, 23 feet.

Electricity. Elec. force. do (corrected.) Lifting power.

°8.....	18°4.....	6.5.....	2.75
1.8.....	41.4.....	14.4.....	10
2.6.....	59.8.....	21.....	23
3.8.....	87.4.....	31.....	45
8.1.....	186	65.....	238
10.9.....	250	88.....	540
4.3.....	99.3.....	670
5.7.....	132.5.....	890
8.6.....	198.7.....	1060
14.4.....	331	1400
21.6.....	497	1800
36	828	2030

44. On one occasion the power necessary to break contact was 2090 lbs. or nearly *nineteen hundred weight*, which is I believe a greater weight than any magnet has hitherto carried, and is certainly vastly superior to the performance of any of the same weight; and I can shew (45. 50.) that this power great as it is, is not *so much* as is due to its peculiar shape.

45. The latter part of the above table was obtained experimentally before the first part, and in the mean time the proper insulation of the coils from the iron was destroyed by an accident, and not having the opportunity of refitting the electro-magnet, I have been obliged to supply the corrections of electric-force seen in the third column and calculated on the basis of the power obtained when the insulation was good. I can place great confidence in these corrections, but must confess that I cannot give that which I suspect to be necessary in (44), I have therefore related that experiment without mentioning the electric force. As however this uncertainty will not materially affect the subsequent observations, and only induces the suspicion that the maximum power of this electro-magnet is not yet attained, I have thought it best to relate that experiment (44) in the absence of the more complete data which I hope to advance in my next communication.

TABLE II.

Electro-magnet, No. 2, (40.) Weight 1057 gr. ; length of wire 7 ft.

Electricity.	Electric-force.	Weight carried.
0.51	30.57	20
1.53	10.7	38.5
6.1	42.7	49

TABLE III.

Electro-magnet, No. 3, (41.) Weight 65.3 gr. ; length of wire 1.58 ft.

0.42	0.66	5.5
1.0	1.58	9
2.0	3.16	11

46. With great care this small electro-magnet supported in one instance twelve pounds, or 1286 times its own weight.

47. No. 4, (42.) the weight of which was only half a grain, carried in one instance 1417 grains, or 2834 times its own weight.

48. It required great patience to work with an arrangement so minute as this last, and it is on this account that the above weight is not nearly so great as it ought to have been, the relative power however which I obtained with it is far greater than any that I had hitherto seen, and is more than eleven times that of the celebrated steel magnet of Sir Isaac Newton.

49. It is well known that the steel magnet should necessarily have a much greater length than breadth or thickness, and that the contrary shape is attended by the confusion of the

poles and a general diminution of virtue, and Mr. Scoresby has found that if a large number of straight steel magnets are bundled together, the power of each when separated and examined is greatly deteriorated.* All this is easily understood, and finds its cause in the attempt of each part of the system to induce upon the other part a contrary magnetism to its own. Still there is no reason why the principle should be extended from the *common* to the *electro* magnet, especially as in the latter case a great and *commanding* inductive power is brought into play to *sustain* what the former has to support by its own unassisted retentive property. All the preceding experiments support this position and I shall give a table in proof of its obvious and necessary consequence,—that *the maximum power of the electro-magnet is directly proportional to its least transverse sectional area*. The first column contains the least sectional areas in square inches of the whole magnetic circuits. The maximum powers in pounds avoirdupois are recorded in the second; and these reduced to one square inch constitute the third, under the title of *specific powers*.

TABLE IV.

	Least Sec Area.	Max. Power.	Spec. Power.
My own Electro-magnets.	No. 1 ... 10	2090	209
	No. 2 ... 0.196	49	250
	No. 3 ... 0.0436	12	275
	No. 4 ... 0.0012	0.202	162
Electro-Magnet at the "Manchester Victoria Gallery," made by Mr. Nesbit: length around the curve about three feet; diameter of iron 24 inches; sectional area, 5.7 inches; do. of armature, 4.5; weight of iron, about 50lbs.	4.5....	1428	317
Prof. Henry's†, of iron, 2-inch square; its sharp edges rounded; weight 21lbs.; length 20 inches round the curve.	3.94...	750	190
Mr Sturgeon's (one of the first exhibited in this country); length about one foot; diameter half an inch.	.196...	50	255

50. These results are, I think, sufficient to prove the rule, if we make an allowance for various sources of error. No. 1, is unfortunately made of a piece of unsound iron, and moreover is suspected not to have been saturated (45.), otherwise I have no doubt that its power per square inch would have approached 300, or, that the whole would have been 6 or 7 hundred weight greater. Again, the specific power of No. 4,

* *Magnetical Investigations*, pp. 37, 38.

† *Silliman's Journal*, vol. xix. p. 404.

is smaller than the mean simply because of the extreme difficulty of making a good experiment with it (48.). With regard to Mr. Nesbit's electro-magnet* the battery used was so powerful (19 of Daniell's two feet cells) and the quantity of conducting wire so very large (14 lengths of wire each 70 feet long and about 1-14th of an inch thick), that its magnetism must have been brought to the utmost possible pitch of intensity, which therefore excelled the mean. On the other hand Professor Henry's for the opposite reason exhibits a specific power much *below* the mean.†

51. The mean of the specific powers of No 2, No. 3, and of that at the "Royal Victoria Gallery" may I think be fairly taken for the expression of the maximum magnetic force of iron under ordinary circumstances, which is simply stated by the formula $x=280a$ where a is the least sectional area in square inches of the magnetic circuit (49.).

52. Since the element of *length* has no place in the above formula and has in fact only a secondary influence playing the part of an active *resistance* (55) which it requires a large additional force to overcome; it is obvious that in the direct ratio of its reduction, will the attractions relative to weight of iron increase. Hence the large power, in this respect, of my short electro-magnets. Hence, also, I have no doubt that a relative power of 10,000 might be attained, and by increasing the sectional area and at the same time diminishing the length, or, what is the same thing and indeed the only means of its performance, by increasing the length and diminishing the diameter of the cylinder (39) of No. 1, that a single pound weight of iron might be made to carry 2 or 3 tons.

53. All this corroborates what I have before stated‡ with regard to the proper construction of the electro magnet for lifting purposes, and it is well illustrated by fig. 4, of plate xi. in the "Annals" for last April: if, in that figure, the line between b and e (which has been omitted by the engraver) be drawn, it will be evident that in the case of saturation when the magnets A and B are brought into contact, the *oblique* forces will vanish, and the attraction will consequently exist in the simple ratio of the smallest number of magnetic particles opposed to each other.

* I have had the pleasure of seeing another electro-magnet of this gentleman's construction. It is short and thick, and consequently adapted for lifting a large proportional weight.

† His coils consisted of nine lengths of copper bell wire, each 60 feet long. The battery consisted of a single pair, which was certainly not sufficiently intense to overcome the resistance of the wire, so as adequately to effect the saturation of the iron.

‡ "Annals" vol. iv. p. 60.

54. With respect to the magnetizing coils, I may observe that each particle of space through which a certain quantity of electricity is propagated appears to operate in moving the magnetism of the bar with a force proportionate to the inverse square of its distance from the surface of the iron, and that when the tension or specific magnetism is the same, the thickness of iron on which that particle of conducting space acts, has nothing (apart from resistance and other foreign circumstances) to do with the whole effect. Now it may be mathematically demonstrated that, such being the law; if each particle induce upon a *large surface*, the resulting magnetic force will not vary much with the distance, but be a very constant quantity for any distance which bears a small ratio to the dimensions of that surface. Hence it is that a coil *within* a hollow piece of iron has no power to magnetize it;* in that case its energy is directed in equal quantities towards opposite directions, the nearness of one surface exactly counterbalancing the size of its opposite. And hence also in the case of my large electro-magnet, where the surfaces are large, every particle of conducting wire would perform its full extent of duty even if it were not quite close to the iron.

55. When the interferences arising from tension are reduced to a minimum by completing the magnetic circuit and making use of a very small electric force, (33,3) the resistance from *length* becomes a very sensible quantity,† varying probably in the direct ratio of that element. Some idea of its character may be formed from the following table, where I have compared half the maximum powers of each electro-magnet with the electric forces (33,3) that produced them; and, by dividing the former by the latter, I have a third column which, under the title of *specific power*, contains the quantity of lifting power (of that degree of tension) due to an unit of electric force.

TABLE V.

	Elec. forces.	$\frac{1}{2}$ max. power.		Specific power.
No. 1.	200°1060lbs.	5.3lbs.
No. 2.	4.5 25	5.5
No. 3.	.66 5.5	9.2

56. The electric force against No. 2 is rather larger than the truth, on account of the greater relative distance of its coils from the iron: if we make on this account a slight addition to its *specific power*, we shall find that the results are in character with the observations in 54, 55, and that the

* "Scientific Memoirs," Part V. p. 14.

† "Annals," vol. iv. p. 59.

specific powers are the same for each, after allowance has been made for the *resistance of length*.

57. It is well known that when the galvanic circuit is broken, the armature is retained in its place with very considerable force. I was anxious to try the capability of my cylinder, No. 1, in this respect, and have arranged my results in a table, the first column of which contains the *degrees* (33,3) which were cut off; the second, the lifting powers due to these quantities of electricity; and the third, the power left after the current was broken.

TABLE VI.

Elec. force.	lifting power.	retentive power.
88°	540	33
29	40	16
14.5	10	10

58. There is considerable difficulty in making a good experiment with so powerful an electro-magnet as No. 1, when very small forces are measured. Nevertheless it is certainly the case that the *retentive is very nearly equal to the lifting power* with small quantities of electricity. Another curious circumstance presents itself in the very inferior retentive power of my electro magnet compared with those of considerable length. It is the natural consequence of its peculiar shape (49).

59. When the whole current is not cut off, but merely reduced by the interposition of a bad conductor, a surprising quantity of magnetism may be *supported* by a very small electric force. I subjected No 1 to 90° (33,3) a quantity adequate to bring its power up to 560 lbs., and then reduced the electricity to different degrees of intensity. Here are the results. The first column contains the degrees of electric force (33,3) to which the superior current of 90° was reduced; the second expresses the weight which is simply due to those quantities; and the third gives the lifting power which the same quantities could *support*.

TABLE VII.

Elec. force.	Lifting power.	Supported power.
31°	45lbs.	294lbs
21	23	210
14.5	10	112
6.2	26	63
4.1	11	56

60. A battery of the size of a common thimble is quite sufficient to produce 31° of electric force, and consequently

to sustain a magnetic power of about 300 pounds, and it is easy to perceive that, by increasing the size of the electro-magnet and the quantity of its conducting wire, the same minute source could support a magnetic virtue of an indefinite amount.

61. I must now conclude my remarks for the present. I intend, however, shortly to construct an instrument of a still larger amount, both of absolute and relative power, than that described in 39; and I will only add that the form I have now given to the electro-magnet is the only one which will permit an unlimited increase of size without diminution of relative power.

Note on Voltaic Batteries.

62. Having had occasion about a year ago to construct a battery of great intensity, it became a great object with me to devise such an arrangement of the elements as should be both convenient in use, and when destroyed, easily refitted. After trying and rejecting two or three systems, I succeeded in producing one which answered my immediate purpose very well; but as I was aware that experience was the only strict test of its value, I have hitherto refrained from presenting it to public notice. Now, however, that I have worked with it during nine or ten months, and have found it to possess every quality that can be desired; I hope in describing it to give the same facilities to others which I possess myself.

63. I have represented a series of three elements in fig. 3. A, B, is the common divided Wollaston's trough with the front side removed in order to shew the inside. The black lines within the cells are rectangular pieces of strong sheet copper, bent on a gauge to the shape seen in the figure. Within these, z, z, z, represent plates of sheet zinc amalgamated in those parts which are in contact with the dilute sulphuric acid, with which I always charge my batteries, and fixed in their places by pieces of hard wood furnished with grooves and extending the whole breadth of the zinc. Lastly, a, a, a, a, a, are pieces of square wood with holes in their centres to admit the screw bolt s, s, which secures the whole.

64. When the battery is worn out, empty its trough and place it therein; then unscrew the bolt and remove it and the pieces of wood; change the old zinc plates for new ones, taking care in the mean time to see that those parts of the copper which touch the zinc are bright; then replace the pieces of wood a, a, &c. pass the bolt through their centres and screw the whole tightly together. In this way I can easily refit three batteries, each consisting of ten pairs, (including the

amalgamation of fresh zinc plates,) in three quarters of an hour.

65. Of course Mr. Smee's battery may be conveniently fitted up on my plan. I prefer however for ordinary use an electro-negative element of *sheet iron* before either copper or platinized silver. In using sheet iron it is well to *tin* that part which is to touch the zinc in order to keep its surface bright.

66. I have lately constructed a large battery on Mr. Sturgeon's plan, and from my experience with it I am convinced that it presents a very superior arrangement of voltaic elements. It consists of eleven cast iron cells each one foot square, and $1\frac{1}{2}$ in. in interior diameter. With eight pairs, arranged in a series of four, I can raise to a full red heat 18 inches of copper wire one tenth of an inch thick.

Broom Hill, near Manchester, 21st August, 1840.

XXV.—Professor VAN KOBELL on a new kind of *Electro-type Engraving, by which, without the use of previously engraved plates, impressions in the manner of Indian-ink Drawings are produced.*—From the *Gelehrte Anzeigen der k. bayer. Akademie d. Wissensch.* Nos. 88 and 89—1840.

It was the great and, indeed, remarkable advantages that practical science has already reaped from Jacobi's application of the galvanic precipitation of copper that first induced me to make the following experiments, and which, to the best of my belief, are new.*

I allude to the precipitation of a plate of copper into the surface of a painting or drawing in the indian-ink manner—the plate thus formed being capable of having impressions thrown off from it in the usual way.

It was easy enough to see that if we could succeed in giving the surface of the colour a conducting power, there might, of course, be formed on it a coating of copper whose minutest details would correspond with our drawing. The kind of drawing in question however, that is to say, sketching on a polished surface makes it necessary to work up the colours employed with some oily or resinous substance, and this does away with its conducting power. Neither can we apply a coat of black lead or other similar conducting substance,

* Most of the plates used by our English calico printers, &c. &c. are, I understand, now produced by galvanic precipitation.—*Translator.*

inasmuch as the most delicate tints and shades of the drawing would suffer from the use of the brush.

I, therefore, set about trying to throw down a coating of copper into a sketch made on a plate of silver without employing any such expedients, for as the copper precipitated by this process is thrown down in a crystalline form, and the aggregation of individual crystals in pure malleable metals readily assumes the forms of plates, (inasmuch as their tesseral forms, when in thin films, so unite together as to form such) it struck me that it would be a mere matter of time to cause depositions on non-conducting places *when interspersed and surrounded with good conductors.*

The experiment bore out my expectations; and drawings in wax, varnish, copying-ink, &c. &c. were covered with a deposit without any conducting power being imparted to them, and this not unfrequently in a very short time. I had frequently occasion to remark how little nodules of copper began forming at the centre of the non-conducting surface with which the lower plate was there entirely coated, these nodules gradually running into each other, and forming lines and threads by subsequent aggregation. As it always requires from four to five days to obtain a plate thick enough to print from, it is the less necessary to impart a conducting power to the colour for the most delicate shades, that is to say, the thinnest films of the paint are, for the most part, completely coated over by the second day, leaving but a few patches free, the closing up of which may be hastened by the application of a coat of good conducting black lead, laid on with a paint-brush, for the drawing in the state in which it then is, is not injured by so doing. Before having thus recourse to the brush, the plate is to be dried with bibulous paper.

With regard to the method of forming the picture we wish to copy, the first thing is, that it should be painted on a bright plate of silver or copper.* The painting is to be executed with a *single colour*, which should be laid on with the clammy oil used in painting on china, and which is the residuum obtained from the evaporation of oil of turpentine. By way of colour, we may use the red ochre of the porcelain painters. A solution of Demerara gum in oil of turpentine, duly thickened by an admixture of red ochre, mineral black, or some

* Copper may be grounded with a coat of whiting, and on this, without difficulty, we may draw with a fine pen, using a solution of sulphuret of potassium (with the maximum of sulphur) by way of ink. The black lines thereby obtained may be removed while yet moist by washing, the drawing being, nevertheless, visibly impressed on the copper, owing to a kind of corrosive action.

such substance, furnishes also a colour that is pleasant to work, and which dries rapidly. The colour is to be so applied that the polished surface of the metal, where left bare, gives the brightest lights, while those parts that are more or less coated furnish the shadows. It is right to observe, that it is noways necessary that the colour should be laid on in a thick coat, *on the contrary, the more delicate and the finer is the execution of the drawing*, the sharper is its re-production in the copper-plate, and the sooner is this completed.

The colour, when dry, should adhere firmly to the plate, otherwise a thin film of copper, which nothing but nitric acid can remove, gets in beneath it. The surface of the colour is not, however, to be quite smooth; it must be fine-grained, otherwise the copper-plate precipitated into it will not take the printing ink.

In some of the experiments I mixed up formate of silver with the colour, and exposed the plate to a gentle heat. Conducting points of silver were thus generated on the surface, whereby the covering of the whole was hastened, but no such addition is, as before remarked, necessary.

With regard to the precipitation of the copper, we may, for that purpose, use Jacobi's apparatus, or we may employ a copper trough with a parchment frame, an arrangement which Professor Steinheil—following up Daniell's plan—has introduced, or recourse may be had to Spencer's arrangement.

The employment of Jacobi's arrangement has this disadvantage, namely, that when the action has been going on for some time, the edges of the plate become too thick, forming rough borders, especially towards the corners; besides, without frequently changing its position, the copper is not precipitated of equal thickness over the whole surface, and it requires a certain degree of practice to prevent the metal running out into lines and branches upon the plate. The use of the copper trough has, it is true, its advantages; by frequent use, however, it becomes so coated with copper that it is necessary to give it a new bottom on account of the wavy form the old one assumes, besides there is more copper precipitated by its use than the operation requires. The apparatus that I have employed, and which I find answers the purpose very well, is composed of a flat-bottomed vessel of china or glass, and whose sides are two or three inches in height. A plate of copper is laid on the bottom of this vessel, having a strip of the same metal an inch and a half in width rivetted to it at right angles by way of conductor. This metal band, with the exception of its upper end, is insulated throughout by a coat of wax.

The dimensions of this bottom plate must be such that it should project about half an inch all round the plate on which the drawing is made, the latter being placed thereon during the operation. At first I connected the conducting strip of metal directly with the painted plate, but the edges of the plate thus obtained were too ragged, and this is avoided by the above modification of the apparatus. Above these plates, and resting upon feet about a quarter of an inch high, there is fixed a frame with parchment stretched across it, or, in other words, a tambourine. In this there are laid a couple of small glass rods, and on them a plate of amalgamated zinc, the metal being thereby prevented from coming into contact with the diaphragm. To establish the connexion, I make use of a copper plate somewhat smaller than the zinc plate, and resting on it, and furnished with a ribbon of copper about an inch and a half wide. This strip of metal either dips down into a channel filled with quicksilver adapted to the strip in connexion with the lower plate, or the two bands are connected by a binding screw. The employment of mercury, in making the connexion, requires care, for if any of it gets thrown into the lower plate as it lies during the operation, a thing likely enough to occur in inserting or withdrawing these strips, there is formed an amalgam with the copper to the destruction of the plate. It does not answer equally well to employ a wire in lieu of the broad connecting strip of metal, for on doing so we find the precipitation considerably weakened. The glass vessel up to the spot to which the frame when inserted comes, is to be filled with a concentrated solution of sulphate of copper, and water moderately acidulated with sulphuric acid is to be poured on to the zinc plate to the depth of a few lines. To keep up the precipitating action of the fluid, crystals of sulphate of copper should be scattered round the copper plate. From time to time I renewed the upper fluid, and replaced the zinc plate when considerably eaten away. Inconsiderable deposits of copper on the parchment may be scratched off, but if they increase to any extent, a new membrane must be used. Instead of such a tambourine, it may be mentioned, we may employ a trough of half-burnt clay, porous enough to allow of the percolation of the fluids, but in this case the precipitation is by no means so rapid.

By following the plan I have recommended, I have, in from four to six days, obtained plates four inches square, and above a line in thickness, and tolerably even throughout. When the surface is waved and uneven I withdraw the plate, and having dried it with bibulous paper, I file it down till it is of uniform thickness. I then replace it and allow the operation

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to proceed as before. Occasionally, also, I have covered particular spots with wax to allow others that were lower to increase to the height of the former, and the plate has then been filed smooth. It is advisable to direct one's attention from time to time to the thickness of the metal so as to turn round the thinnest edges of the plate under the parts of the diaphragm where the action is the strongest. To endure a rapid and compact precipitation, it is above all things requisite that the solution of copper should be constantly maintained at the point of saturation. The bubbles of air that adhere to the plate on its first immersion may be removed with a camel-hair pencil. It is only at the commencement of the process, that is to say, till the picture is coated over, that the operation demands our attention.

When the plate has attained the desired thickness, the edge all the way round is to be filed away, upon which, generally speaking, the two plates separate without difficulty. To render the plate we thus obtain fit for furnishing impressions, all we have to do is to clean off with æther any particles of colour adhering to it.

The impressions have the appearance of Indian-ink drawings, and the tone of colouring is extremely delicate, a fact the painter ought not to overlook.

I think that the accompanying specimens will* bear me out in the idea that this modification of the electrotype process is the more deserving of the attention of artists as it enables them, and that without much previous knowledge on the subject, to throw off copper-plate impressions of any sketch or picture. It need scarcely be remarked, that the graver may be subsequently applied to a plate thus produced, supposing we wish to heighten the effect of any particular part of the engraving. From what has been said, it will be seen that the process is by no means an expensive one.

Translated by
W. G. LETTSOM, Esq.

* We have several beautiful specimens of printing from this style of electro-type, which were sent to us with this paper; also one electro-type plate, with the picture of a tree, from which we give a copy in the electro-type plate.—EDIT.

XXVI.—On the Analysis of Limestones, especially the Magnesian kind, and a method of completely separating Lime from Magnesia, when both are present in large quantity. By ROBERT E. ROGERS, M. D. and MARTIN H. BOYE'.

Carbonate of lime, associated with more or less carbonate of magnesia, forms the principal ingredient of limestones. In some varieties the latter substance appears only as a trace, while in others, it amounts to nearly 50 per cent. of the mass. When the proportion of the carbonate of magnesia is very considerable, the rock is termed magnesian limestone, or dolomite, the latter name being mostly applied to the crystalline varieties. Variable quantities of other substances, as silica, alumina, and the oxides of iron, and manganese, are generally associated to some extent with the above principal constituents.

The Silica is usually either in the free state, in the form of small transparent grains of quartzose sand, sometimes imperceptibly minute, or in chemical combination with the alumina and iron, (clays, &c.).

The extensive use made of limestones in the arts and agriculture, as mortars, cements, fluxes and manures, renders it a matter of great importance to procure a certain and expeditious process for their analysis, especially as there exists great diversity of opinion respecting the relative efficiency of the several constituents.

We proceed to describe a mode of analysing calcareous carbonates, which we have found in practice both certain and expeditious, and, therefore, preferable, we conceive, to the methods generally in use, which demand extreme care and considerable time to furnish accurate results. The method here proposed, we have adopted with success in an extensive series of analyses performed for the geological survey of the state.

The limestone is first finely powdered, when a given weight, about 30 grains, is digested in chlorohydric acid, in the ordinary way, evaporated to dryness, moistened with chlorohydric acid, and re-dissolved and filtered. The silica and a large part of the other adventitious substances are thus left upon the filter. They are then calcined and weighed, a correction being made for the weight of the ashes of the filter. These steps give the amount of the *insoluble matter*.

The filtered solution, containing besides the lime and magnesia, portions of alumine and oxides of iron and manganese,

* Journal of the Franklin Institute.

is neutralised with ammonia, avoiding an excess, and then precipitated with sulphhydrate of ammonium, a small quantity of which will usually suffice. When the precipitate has subsided, it is filtered, the funnel being covered with a glass plate, so as to exclude the atmosphere, and then washed with water containing a few drops of the sulphhydrate of ammonium. The filter, with its contents, is then removed, pressed between bibulous paper, dried and calcined. The alumina, and oxides of iron and manganese are thus obtained together. When their quantity is such as to require them to be separately estimated, it can be done in the ordinary way.

In determining the lime and magnesia, a fresh equal portion of the powdered mineral is employed, which is decomposed by a sufficient quantity of dilute sulphuric acid, with the aid of heat. Water is then added so as to fill the vessel up to a given mark, after which alcohol of known strength is introduced in such proportion as to make the whole solution contain 40 or 41 per cent., (estimated by volume) of alcohol. The alcoholic solution of this strength * precipitates entirely the *sulphate of lime* along with the insoluble matters. When the precipitate is settled, it is filtered under cover of a glass plate, and repeatedly washed with dilute alcohol of the same strength, as that previously employed, until a barytic solution indicates no trace of sulphuric acid. The whole is now calcined, and the weight of the insoluble matters as already ascertained, being deducted, we obtain the amount of *sulphate of lime*, from which we compute that of the *carbonate*.

The filtered solution now contains the sulphate of magnesia, and an inconsiderable portion of the sulphates of alumina, iron, and manganese, besides an excess of sulphuric acid.—It is to be evaporated until all the alcohol is dispelled, and then precipitated by pure carbonate of potassa, with the precautions usually prescribed. The magnesia, alumina, and oxides of iron and manganese, thus precipitated, are filtered, washed and calcined. Subtracting from the weight of the whole, that of the three latter previously ascertained, we find the amount of the magnesia, which is to be estimated as carbonate.

The separation of the lime in the form of sulphate from magnesia, by an alcoholic solution, is so complete as to make it unnecessary to estimate directly the magnesia, except when we desire to check one result by the other. The above pro-

* Alcohol of this strength has a *specific gravity* of 0.951 to 0.949 at 60° Far and marks between 17° and 18° Baume. Alcohol of the shops (alcohol rectificatus Lond. Phar.) marking 54½ Pennsylvania proof—has a *specific gravity* of 0.835. Five volumes of this, and 6 a 6½ volumes of water, will give a very suitable mixture for the above purpose of analysis.

cess, it need hardly be said, will apply equally to the analysis of other substances than limestones, in which lime and magnesia abound, for we have only to precipitate these earths as carbonates, convert them into sulphates, and then treat them with dilute alcohol after the manner described.

We present the following analyses by way of illustration:—

1. A white crystalline dolomite, from the neighbourhood of Montville, New Jersey. *Specific gravity*—2.853

A portion, 1.469 grammes, was raised to a dull red heat, and the water, which was received in a tube containing chloride of calcium, was found to weigh .007 grm. or .48 per cent. This small amount of water is not expelled at the temperature of boiling water.

Two other portions of the powdered mineral, treated after the manner described, gave these results:—

	Per cent.
Insoluble matter.....	.04
Alumina, ox. iron, and ox. manganese.....	.15
Sulphate of lime, and insol. matter.....	76.09
Magnesia, alumina, and ox. of iron and manganese	20.70

By subtracting the insoluble matter .04 from the joint weight of the soluble matter and sulphate of lime 76.09, we get 76.05, and subtracting the alumina and oxides of iron and manganese from the joint weight of these and the magnesia, we have for the magnesia 20.55

A reference to the annexed table, the use of which will be explained, shews that 76.05 per cent of sulphate of lime is equivalent to 31.54 per cent. of lime, or to 56.11 per cent. of carbonate of lime.

It also appears that 20.55 per cent. of magnesia is equivalent to 42.54 of carbonate of magnesia. The result will, therefore, stand thus:—

Carbonate of lime	56.11
Carbonate of magnesia.....	42.54
Alumina and oxides of iron and manganese.....	0.15
Insoluble matter.....	0.04
Water	0.48
	<hr/>
	99.32

Were we to estimate the magnesia in this case by the loss, it would be 43.22 carbonate of magn. equivalent to 20.88 magnesia, or 0.33 per cent. more than the amount found by direct estimation.

With a view further to shew that the *whole* of the lime is procured by the above method, and therefore, that we may safely estimate the magnesia by subtracting the carbonate of

lime and other ingredients directly got from the weight of the mass, we subjoin the following example of a specimen found to contain no magnesia.

2. A white crystalline, imperfectly saccharoidal limestone, from near the mouth of Yellow Breeches Creek, Susquehanna River, Pa.

From one portion of the powdered mineral, treated with chlorohydric acid, we obtained

Insoluble matter 2.3 per cent.

Alumina..... 1.2 „

Ox. of iron and manganese..... none

Another portion treated with sulphuric acid and diluted alcohol of the proper strength, gave

Insoluble matter and sulph. lime. 133.19

*Table for calculating lime and carbonate of lime from the magnesia from magnesia or its sulphate**

		1	2
Sulphate of lime	Lime	0.41532	0.83064
Sulphate of lime	Carbonate of lime	0.73780	0.47561
Magnesia	Carbonate of magnesia	2.07002	4.14004
Sulphate of magnesia	Magnesia	0.34015	0.68030

The first vertical column of the table contains the names of the substances, from a known weight of which we wish to compute the weight of the substances embraced in the second column. The figures in the horizontal lines represent the quantities of the substances named in the second vertical column corresponding to those quantities of the substances in the first column, which are signified by the numbers at the head of each vertical division of the table. An example will render the mode of using the table sufficiently plain.

In the first analysis, the amount of sulphate of lime was 76.05. To ascertain from the table the quantity of carbonate of lime equivalent to this amount of sulphate, we find on the horizontal line appropriated to the carbonate, the quantity due to seven parts of the sulphate—namely, 5,16463, then the quantity due to six parts, namely, 4.42682, and then that equivalent to five parts or 3.68902. By arranging these in their proper decimal order, so as to impart to the several

* This table is taken partly from H. Rose's Analytical Chemistry, vol. ii., and partly calculated for the present purpose. The principle of this method of calculating analytical results was first set forth by Poggendorf, in his *Annals*, vol. xxi., and has since been extensively carried out by H. Rose in his work just mentioned.

† The Table in the middle is to be read through both pages.—*EDIT.*

Subtracting the insoluble matter, 2.3, from the sulphate of lime and insoluble matter, we have sulphate of lime 130.89 per cent., which is equivalent, as the table will shew, to 96.3 per cent. of carbonate of lime.

The amount of water as derived from a third portion was 0.2 per cent. Our analysis therefore stands thus :—

Composition in 100 parts—

Carbonate of lime	96,3
Carbonate of magnesia.....	none
Alumina	1.2
Insoluble matter.....	2.3
Water	0.2
	<hr/> 100.0

sulphate of lime, and also for calculating the carbonate of

3	4	5	6	7	8	9
1.24596	1.66128	2.07660	2.49102	2.90724	3.32256	3.73788
2.21341	2.95121	3.68902	4.42682	5.16462	5.90242	6.64023
621006	8.28009	10.35011	12.42013	14.49015	16.56017	18.63019
1.02045	1.36060	1.70075	2.04009	2.38105	32.72120	3.06135

amounts taken from the table, the value they are intended to have as units, tenths, hundreths, &c., and then performing a simple addition, we get the amount of carbonate corresponding to the whole quantity of the sulphate.

The figures will stand thus :—

Sulphate of lime	76.05
	<hr/> 51.6462
	4.42682
	.000000
	<hr/> 368902

Carbonate of lime 56.1099102

In the same manner, 20.55 of magnesia will be found to be equivalent to 42.54 of carbonate of magnesia—thus :—

Magnesia	20.55
	<hr/> 41.400
	00.000
	1.035
	<hr/> 1.03

Carbonate of magnesia 42.538

As it may be sometimes convenient to evaporate the magnesian solution to dryness, ignite it, and from the sulphate of magnesia thus procured, compute the magnesia or its carbonate—we have introduced into the table a column to facilitate the calculations.

XXVII.—*Extract of a Letter from W. Snow Harris, Esq. F.R.S., to Mr. W. Sturgeon.*

The simplicity and convenience of my plan of fixed conductors having been in the year 1820 generally admitted, the Navy Board were led to institute some further inquiries into the general effects of lightning on ship-board, and I was called upon to shew that the connexion of my conductors *with the sea* through the metallic masses in the hull was in no way detrimental to their action, or liable to objection as involving any danger to the vessel—the electrical discharges might as safely become dispersed this way as by a lightning chain hung in the rigging, perhaps more so, considering that these conductors were massive and continuous, and linked with the various metallic masses in the hull and sea into one general whole.

I was further called upon to explain what had been the ordinary course of lightning on ship-board, and what would, in all probability be the effects of electrical discharges upon my conductors.

In order to meet the views of the officers of the Board, as made known to me at that time, I naturally enough resorted to such practical experiments and observations as were within my reach, and calculated to bear immediately upon the points in question: I cited numerous instances of ships struck by lightning, in which heavy discharges had been safely transmitted to the sea through the intervention of the keelson bolts and other metallic bodies passing through the hull, and which were shewn to have been of such frequent occurrence as to lead to a common observation among sailors, recorded in the Philosophical Transactions, that when the lightning had reached the well the danger was over. By way of shewing the operation of *my conductors through the hull*, I resorted to the experiment you first loosely notice—strong charges from twenty-five square feet of coated glass were passed over a vessel's masts, fitted with the conductors, so as to shew the perfect facility with which the charge pervaded the hull and the sea at the same time: the charge was adequate to the fusion of 15 feet of small iron wire—percussion powder was placed over

the joints of the conductors on the mast, and the sliding masts were put in motion at the time of the passing of the charge, and placed in different positions at each repetition of the experiment. I believe, any one must perceive, that the experiment shewed—1st, The perfect operation of the conductor through the hull. 2nd, Its continuity. 3rd, Its complete operation, under every possible position of the mast, which was required to be done.

You, however, shut your eyes to these plain deductions, and tell your readers that the experiments prove nothing peculiar to my system of conductors, and serve only to shew that copper is a conductor of electricity, and that detonating powder can be ignited by an electric spark; and this is what you call giving “a fair and candid explanation of my experiments before the Navy Board at Plymouth.” Now, I never asserted that any other conductor *would not* convey an electrical charge to the sea. My experiments were never *instituted* under such an impression: they shewed, however, the continuity of the copper plates along the masts in the way I had disposed them; for had detonating powder been placed in a similar way about the *conductors then in use*, it would have inflamed, if exposed to a similar charge. These experiments were subsequently carried out in the Thames, opposite Somerset House, and again at Plymouth, on a very extensive scale, and in various ships of the navy, and were admitted by all who witnessed them to have an important bearing on the question of marine lightning conductors.

You inquire, whether you have not pointed out other experiments with which *I ought* to have made the Navy Board acquainted? Do you, then, really imagine that the simple facts to which you allude, and which are known to every tyro in electricity, were not also well known to the officers of the Board?

Are you serious, when you say, I ought to have shewn the officers that a wire *heated RED-HOT* by electricity *would ignite gunpowder*? and that an interruption in the conductor, by a cut of a saw, would cause an electric spark in the opening? However ignorant you may suppose the officers of the Board to have been of this subject, they certainly understood the matter very much better than you *appear* to do; they did not require such horn-book information: they entered very fully into the merits of the question, and left no point unexplored. They required of me information respecting the relative conducting powers of different metals; their respective resistance to fusion by electricity; the ratio in which they became heated either by the same or different quantities of electricity; the

quantity required to heat wires of different diameters to the same degree, &c. &c. Experiments for the perfect elucidation of which, in meeting their views, I was obliged to invent new electrical apparatus, and exhibit the results in a way not before done.

I come now to the experiment you have characterised as shewing nothing more than the effects of gunpowder "blowing asunder two pieces of wood." This experiment was made to meet the inquiries of the Board as to the effect of a conductor incorporated with the mast, in *confining the discharge to its surface*, and preventing it *from entering the wood*.

My first illustrations were confined to small models about four or six inches in length, which could be *splintered* by the force of a heavy battery, and *saved* from damage by the application of metallic leaf along the surface; but being desirous to exhibit the same result on a larger scale, and shew how completely the *surface* conductor directed the charge without entering the interior, I tried the experiment under new and very delicate circumstances. A model of a mast, about ten feet in length, was made in parts, and an interrupted line of metal passed *through* it, percussion powder being placed in the interruptions; a continuous conductor was attached to its *exterior*, and *both* connected at a common point of junction outside the model, so as to give the electrical shock the choice of passing in the direction of *either* or *both*. *In no case* did the heaviest explosion *enter* the mast whilst the *exterior* conductor remained. In fact, it was safe both from a direct, and what you call a "lateral discharge." This result being first exhibited, the *superficial conductor* was removed, and a similar charge passed in order to shew by the explosion of the powder within, that the reason of *its failing to explode* in the former case, *was owing to the presence of the exterior conductor*.

Now this result, bearing so directly on the application of conductors to the masts, and which every one will, I imagine, deem conclusive and fair, you *briefly* caricature as the "*blowing asunder two pieces of wood by gunpowder*," and accuse me of endeavouring to persuade the British Association that it is a "fair representation of the effects of lightning on a ship's mast;" and *this you call also an "explanation" of my experiments*. I can only account for such gross misrepresentation by supposing what, I believe, is after all, not far from the truth, that you are really *ignorant* of the subject on which you have attempted to write.

Thus in sec. 202 of your memoir, you say, "this kind of lateral discharge will always take place when the vicinal bodies

are sufficiently capacious and near to the principal conductor which carries the primitive discharge, or *to any of its metallic appendages*;" and in sec. 198 and 199 you say, that with a small jar of a *quart* capacity only, "you can produce lateral discharges, half-inch long, and *at a distance of 50 feet* from the *direct discharge*. That a discharge from such a jar *would imitate* a flash of lightning striking a similar conductor on a mast." In my experiment, which you so much abuse, we have actually *all the conditions* you yourself point out as essential to the exhibition of your own results, supposing them to be according to the course of nature. How then does it happen that we can pass the heaviest "primitive discharges" along the exterior conductor *without in any way effecting the detonating powder within*? If what you say be true, the model should be blown asunder *in consequence of the discharge passing down the exterior conductor*. However much, therefore, you may choose to detract from this experiment, it is by your own admission *consistent* with the course of nature. What would you have more?

These experiments, to which you have alluded, form only a part of those originally employed—there were a great variety of others, such as the dispersions of strips of leaf gold in certain directions only, when placed in the same relative position as the conductor on the mast—the expansive effects of the charge on various bodies—fusion of wires, and such like. In short, the series was as complete as could be desired: the experiments were examined by a Committee of the Royal Society, by Sir Humphry Davy, Dr. Young, and Dr. Wollaston; the latter entered minutely into the matter, and in a letter to the Comptroller of the Navy, gave them his unqualified approbation. *I suppose Dr. Wollaston's judgment will be considered at least equal to your own.*

You will excuse my entering into *the detail* of my kite experiments. Having been for the last ten years an observer of atmospheric electricity, and having had an atmospheric conductor leading into my study, it would be remarkable, indeed, if I had allowed the common electrical kite to have escaped me.

Do you wish "to persuade yourself" into a belief that *your occasional amusement with this piece of philosophical apparatus* entitles you to become a philosophical dictator?

With whatever self-complacency you may regard your employment of the kite, the effects you mention are *very commonplace*, and are *as distinct* from the effects of a concentrated flash of lightning striking a ship, in the way described by Lieutenant Sullivan, of the "Beagle," as it is possible to be.

You ask me to what kind of electrical action I attribute the

bursting of the hoops in the "Rodney," &c. To no electrical action at all, *properly so called*; but, as Priestly has already shewn, to the effects of sudden expansion. He says, "the cause of this dispersion, &c. &c. of bodies in the *neighbourhood* of electrical explosions is not their being suddenly charged with electrical matter, but the air being displaced suddenly, gives a concussion to all bodies that may happen to be near." Did you never see such effects illustrated by artificial electricity? Why, even children who use an electrical machine as a toy, are acquainted with the propulsion of a small ball from an ivory mortar, by the expansive force in the surrounding air caused by a dense spark. Surely, the experiments detailed by Cuthbertson, to shew the bursting open of wood and other bodies, by the expansive effects of the electrical explosion, must be known to you.

One would almost imagine by your putting such a question, that you were really uninformed upon some of the commonest experiments in electricity.

Your question, whether I think it "more prudent to lead lightning into a ship or keep it out," is a plain piece of sophistry. If meant as an argument against my method of *equalizing* the electrical action upon the general mass of the hull and sea, is as deficient as any thing can be. It is, like many other similar efforts in your memoir, *a deceptive and sorry appeal to the fears and prejudices of the IGNORANT*, by imposing upon their credulity, and leading them to imagine that my conductors lead lightning in an *explosive* form into the ship, and deposit it there as so much cargo, than which nothing can be more fallacious: almost every one acquainted with electricity knows, that the great use of a lightning conductor is to equalize, in a rapid way, dense electrical discharges, and so rob them of their explosive power by taking down their tension.

So far, therefore, from my conductors leading lightning into the ship in the way *you would have it supposed*, they virtually come under the "prudent part" of your question, and keep off the explosion altogether, by depriving the charge of its mischievous tendency directly it strikes any where upon the conductors aloft. Now, it should never be forgotten as an important feature in this discussion, that whenever we set up an artificial elevation on the earth's surface we do, in fact, *set up a conductor of electricity*, that is to say, a lightning conductor. The *masts*, themselves, therefore, *are already* lightning conductors, passing necessarily into the body of the vessel, and upon these discharges of lightning will fall, whether detached metallic bodies be present or not, or whether

they be furnished with metallic conductors or not: this is proved by experience. The mast of a ship from its position alone, necessarily determines a discharge of lightning upon the hull. Now, by perfecting the conducting power of the masts, and connecting them with all the metallic masses in the hull and with the sea, we so complete the conducting power of the whole, that an instantaneous distribution takes place in all directions, directly the explosion strikes the mast head, and the electricity is changed immediately, from a dense form, into electricity of comparatively little tension.

Now with respect to the actual results of the trials of my conductors. Have not these conductors, been tried in 12 ships of the navy for as many years? have not these ships been in all parts of the world? have they not all been exposed more or less, to severe storms of lightning?

Do not some of the officers who commanded them, and others, experienced men, *insist* on the *fact* of their ships having been struck by lightning in the usual way *without damage*? Convinced of the protection my conductors afford, have not the captains of ships fitting for service, repeatedly applied to the Lords of the admiralty to be furnished with them? *Can you point out any instance*, in which *inconvenience or damage* has arisen in these ships during this lapse of time? The main argument of your question, therefore, is really answered by the results of experience; if you have *not any good fact* to oppose to these, of what avail is any *theoretical* objection to the use of my conductors you may find it *CONVENIENT* to set up. It must be quite apparent, that my method of defending shipping from lightning is based on admitted principles in *science*, and is, consequently, as free from theoretical objection as any other method. A lightning rod as a defence from lightning, is, under any form, nothing but a means of rendering more efficient the conducting power of the general mass—so as to admit of such intense discharges being readily dispersed, which would otherwise by causing an *explosive expansive force*, produce damage.

According to the Experiments of the learned Mr. Cavendish, the chances of escape from Lightning is in *this way*, increased by at least four hundred million to one, even with a conductor of iron.

The letter in your Annals, signed W. Pringle Green, is really not worth my notice. His ridiculous queries have been so often before the public so often answered, that I cannot really notice them again. I must decline all intercourse with him in the shape of correspondence, and for this plain reason—I cannot place the slightest confidence in any thing he

advances. But, lest I should be thought harsh in making this assertion without apparent truth, I will give a few examples of his respect for accuracy, and I will leave it even to your "candour" to say how far I am right.

1st. In the *Mechanic's Magazine*, vol. VIII, page 286, and in other places, Lieutenant Green states that the conductor of St. Paul's Cathedral, the *largest* ever put up, was, by a moderate flash of lightning, heated *red hot*, and *therefore judiciously removed as DANGEROUS and USELESS.*

That the conductors of St. Paul's church have been removed is a most shameless assertion. Neither have we good evidence for supposing it to have been made *red hot*—this I have shewn in my papers in the *Nautical Magazine*—at all events, if it had been, it could not have been from a moderate stroke of lightning, as stated by Mr. Green.

2nd. In the *Mechanics' Magazine*, vol. viii. page 13, and in a variety of other places, such as the public newspapers—in a pamphlet by himself, Lieutenant Green states, H. M. Ship "Kent" and "Perseverance" were struck by lightning and damaged, although having *conductors at the time.* In the "Kent," he says, three men were killed and several wounded, and the masts much damaged. At this time, he says, "two conductors were up, and there were more than 20 sail of H.M. Ships in company, and *near the Kent, without conductors, none of which were injured.*" Not finding a word about the conductors in the ship's log, I remonstrated with Lieutenant Green on the subject, when he again repeated the assertion at page 287, where he says, that he named the captains of the ships, and that "more than one hundred officers and a thousand seamen witnessed the fact."

The subject having been investigated by the Admiralty Committee, it appeared by letters from Captain Godfrey of the navy, and Admiral Cardon, the former in the "Kent" at the time, the latter in the "Perseverance," that these statements made by Lieutenant Green *are utterly unfounded.* Captain Godfrey says, that the Kent usually had the conductors up; but having been damaged, they had been laid aside. Admiral Cardon, who was in the Perseverance, also affirms, that they *had not a conductor in the ship.* Lieutenant Green talks of "imposing gross deceptions on the naval service and the public." Pray, what does he call this?

3rd. In his letter in your last Number, page 230, he refers to the *Naval Chronicle*, vol i. page 201, as evidence to shew, that my plan has been copied from Mr. Marrot. Now in the first place, there is no mention made there of any such person; and secondly, the memoir to be found there by the celebrated

Frenchman Le Roy, does not contain one word about lightning conductors fixed in the masts.

4th. In the same letter, and the same page 330, Lieutenant Green says, "by my representation of these facts, the existing Board of Admiralty, countermanded the Navy Board's order, &c." In other places e. g. in his pamphlet above-mentioned, in the *Mechanics' Magazine*, 288, he takes credit to himself for having through his influence with the Board of Admiralty, caused my plan to be laid aside. In order to ascertain if such were really the case, I wrote lately an official letter to the Board, referring to Lieutenant Green's assertion in your work, and in other places. The following is the copy of the letter received in reply:—

Admiralty, 6th March, 1840.

SIR,—In reply to your letter of the 3rd instant, I am commanded by the Lords, Commissioners of the Admiralty, to acquaint you that it does not appear by the records of this office, that their Lordships were in any way induced to lay aside your lightning conductors, by any representations of Lieutenant Green, or any other persons, and that that officer is not authorized to make such statements.*

I am, sir, your humble servant,

W. SNOW HARRIS, Esq. Signed, JOHN BARROW.

5th. Lieutenant Green states page 232 of your last number and elsewhere, that my conductors are led through the after magazine†—this he has always insisted in, and has given a drawing to that effect. Will any of the thousands who have been at sea in the ships fitted with my conductors say that this is true?

These are a few of the numerous deceptions which appear in Lieutenant Green's productions. I do not think it worth while to cite any more. A brief notice of his style of reasoning and I have done.

In the *Mechanic's Magazine*, vol. viii. page 14, in order to shew the danger or conductors, he states, that the setting up of certain pointed rods in Lausanne, in 1825, was the cause of a terrible storm which happened there in 1824—that is just one year *after* the storm happened. This logic is based upon extracts from newspapers, in which the dates are given, and by what he calls an explanation in page 295, he makes the storm happen three years *before* the rods were set up, which *he* says *was the cause of it*.

* See *Mechanic's Magazine*, vol. VIII, page 237.

† Whatever might be the cause for discontinuing Mr. Harris's conductors in several men of war which had been furnished with them, it is a fact that such was the case, as will appear by the appendix to this letter.—EDR.

In a newspaper called the *Nautical Register*, in which he wrote against my conductors in 1822, amongst a most luxurious variety of contradictory matter, he has this remarkable passage: "The following statement will bear me out in what I have advanced, that *no man ever did, or will exist, who can invent anything to guard ships from the direful effects of lightning.*" He goes on to say:—"In the year 1801 or 1802, H. M. Ship *Cleopatra* was at anchor about 30 miles from Vera Cruz; early in the evening it commenced to rain, with thunder, &c. The *conductor* was ordered by the captain to be hoisted at the mizen mast head, and from the time of its being hoisted until the morning did *streams of electric fluid continue to run down it into the sea.*" Well, was the ship injured? Not in the least. Still this is to prove no man *did*, or ever will *invent anything* to guard ships from the direful effects of lightning!!!

I am, Sir, your obedient servant,
WILLIAM SNOW HARRIS.

Plymouth, March 10, 1840.

APPENDIX.

Copy of a correspondence with Rear Admiral Warren, Admiral Superintendent of the Plymouth Dock Yard.

"Plymouth, 13th March, 1838.

"Sir,
"As several of her majesty's ships fitted with my new lightning conductors have been paid off at Plymouth, and their spars returned to the dock yard, I should be much obliged by your informing me whether the conductors still remain in them? Whether any having the conductors have been re-issued? Whether, in the case of their having been removed from any cause, they have been refitted in another ship, or have been duly set aside for that purpose? as also, whether any spars with the conductors in them are yet remaining in store.

"I am, Sir,
"Your very obedient servant,
W. Snow Harris."

"Plymouth Yard, 16th March, 1838.

"Sir,
"As requested by your letter of the 13th instant, I beg to enclose to you a report of the particulars therein required, respecting the spars in store, fitted with lightning conductors, on the plan suggested by you.

"I am, &c. &c.

(Signed by the Admiral Superintendent.)

Mr. W. Snow Harris's *Letter to Mr. W. Sturgeon.* 217

Copy of the Report to the Master Shipwright.

"Plymouth Dock Yard, 15th March, 1838.

"Sir,

"In reference to the questions contained in Mr. Harris's letter of the 13th instant, I beg to state:—

"1st. That the conductors in the spars of ships paid off at this port have been removed, with the exception of five top-gallant masts returned from the Forte, which are now in store, the conductors remaining in them.

"2nd. No spars have been re-issued with the conductors remaining fixed.

"3rd. The conductors which have been removed, from whatever cause, have not been refitted to other ships, but returned into store in common with other old copper."

"Mr. Harris's fourth question appears to be answered in the first paragraph of this memento."

"(Signed) J. F. Hawkes.
J. Shaw."

"To the Master Shipwright."

In consequence of this correspondence, I addressed the following letter to Sir J. Barrow, who had previously favored me with an interview on the subject.

"Plymouth, 10th March, 1838."

"My dear Sir,

"I wrote to admiral Warren soon after my return. You will soon see by the copy of the correspondence herewith transmitted, that the new lightning conductors have been, with a few trifling exceptions, all torn out of the masts and thrown by in a somewhat contemptuous way as old copper: thus, the plates which might have been very well replaced in other ships, have not even been taken care of. The correspondence with admiral Warren is very brief, and will not cost you five minutes attention."

"After the explanation you were so good as to favor me with respecting conductors, I cannot but believe you would wish to have me fairly dealt by in this matter; and I should hope that the Board would not, on a review of the facts, treat me ungenerously. Let us then see in *as few words as possible* how the matter stands in relation to the Admiralty, the country, and myself."

1st. It is an admitted fact, that ships may be *burned and destroyed by lightning*; the logs of the navy shew that this is by no means improbable, and that some missing ships may have perished from this cause. They exhibit a loss of life, of damage, and loss of services of ships at critical periods, not generally appreciated: well then, this subject has been deemed of sufficient consequence to engage the attention of scientific persons for more than half a century, and some steps have been taken to palliate the effects of lightning on ship board. The methods proposed have been *inadequate in some way* for the damage has continued up to the present time; notwithstanding that buildings on land have been protected from this source of danger."

"2nd. In the year 1820 I investigated *practically* this question, and shewed how the fixed continuous conductors of Franklin might be rendered available on ship board, and how by a perfect system of conduction throughout the hull, all the protection which could possibly be obtained from admitted scientific principles, would be arrived at."

"3rd. My proposals were eventually carried into effect in eleven ships of the navy, and the results has been as perfect as could be hoped for. The written testimonies of officers in command of the ships, prove that they have been exposed to heavy thunder storms; that they have been actually struck by the electric fluid, without in any case receiving the slightest damage: thus, not only shewing that the conductors are *unobjectionable*, but actually useful."

"4th. The conductors not only stand upon this, but they are supported by the avowed opinions of some of the most talented men in science the country

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has to boast of : almost every naval officer, to whom the conductors are known is desirous to have them, and many have applied for that purpose ; and this feeling prevails even with the sailors who were at sea in the ships fitted with them, as, for instance, in the *Beagle* and *Dryad*."

"4th. In the face of all this how does the matter actually stand at this present instant ? why thus, the ships in which the conductors were fitted. have been nearly *all paid off*, the plates, *have been commonly torn out of the masts, and thrown by as old copper*, and no notice taken of it. A great national experiment has been *abandoned*, and the results lost to the country, without any assignable reason, without enquiry. An experiment of great consequence to our commercial and naval prosperity, and one which has occupied the attention of the scientific part of Europe for upwards of 70 years."

"Can the affair possibly rest here ; I am sure this could not be the serious intention of the Board ; nevertheless, such is the actual state of the question in relation to the Admiralty and the country."

"6th. In respect to myself, I must necessarily feel the circumstances above detailed to be very severe, and uncalled for by any thing on my part : it is always difficult to speak of one's self ; there are however some cases in which we are called upon to do so ; this appears to be one of them ; and if it be done with becoming diffidence, I trust you will excuse it."

"It is well known, that so far as ability has enabled me, I have for many years cultivated with great zeal, experimental science ; and have not spared *time, toil, or money*, in doing so, as I believe my papers in the 'Philosophical Transactions' fully shew ; indeed, the Royal Society marked their sense of my contributions to the pages of the 'Transactions,' by awarding me their Copley medal in 1835. Many of my researches in electricity and magnetism have been of practical advantage to the navy ; I may claim therefore, at the hands of the Board some little attention."

"Now, in perfecting the application of conductors in ships, I have incurred, not only a very serious responsibility, but a very heavy expense. You cannot but believe, that if any damage had happened, either to the ships fitted with the conductors, or even to the buildings at the Victualling office at Plymouth, (which I should remark, were protected from lightning under my direction,) I must have been the *person* held responsible with the public. Is it right that one who successfully labors to promote the national science, and whose services have been advantageously used for the general good of the navy, should be passed by with coldness and neglect ? Here are these conductors, notwithstanding the many documents and facts conclusive of their value, thrown unceremoniously aside as old copper, and no notice taken of it : surely, without any claim I may have the consideration of the Board on the ground of general science, this it must be admitted has the appearance of dealing somewhat unjustly by me. I cannot but believe, that in stating thus freely all I have to say to you, I am appealing to one who has himself done much for the literary honour of our country, and who, anxious for the advancement of natural knowledge, must necessarily feel well-disposed to promote an enquiry into such a case. When we consider the resources of this powerful nation, and how much its interest is involved in its naval and commercial prosperity, it surely cannot be on account of a thousand or two pounds that an invention of practical advantage to the navy is laid aside."

"I trust you will be so good as to bring this matter under the consideration of the Board, and will do me the justice to believe, that I desire nothing which may not come fairly and openly before the country, without any kind of reservation whatever."

"I am dear Sir, &c. &c."

W. SNOW HARRIS.

Sir John Barrow made a courteous reply to this communication : the matter, however, eventually terminated in nothing more than the fitting of the *Atleon*, without my knowledge, in the way before explained, sec. 19, page 13.

Mr. W. Snow Harris's *Letter to Mr. W. Sturgeon.* 219

Copy of correspondence with the Admiralty, on the subject of an extract from a report on the new conductors to the Admiralty, from the Officers of the Plymouth Dock Yard.

"Admiralty, 12th December, 1837.

"Sir,
"With reference to former correspondence upon the subject of your lightning conductors, I am commanded by my Lords Commissioners of the Admiralty to transmit to you the accompanying *extract* of a report from Plymouth Dock Yard, relative to the state of the masts of the Caledonia, in which the conductors were fitted.

"I am, &c. &c.,

"C. WOOD."

"W. Snow Harris, Esq."

Extract of a report from the Officers of Plymouth Dock Yard, dated 6th December, 1837.

"We beg to acquaint you that the conductors have been removed from all the spars returned from the Caledonia; that the main-top mast has been converted to a brig's main-mast; the fore and main-top gallant masts have been appropriated to jury gear; and that owing to the scores left in the spars by the removal of the conductors, it will be necessary to reduce them before they be re-issued."

"Plymouth, 16th December, 1837.

"Sir,
"I feel much indebted to the Lords Commissioners of the Admiralty for the extract of the report from the Plymouth Dock Yard, relative to the state of the masts of the Caledonia, fitted with my lightning conductors; and hope to be permitted to offer the following remarks on it, which their lordships will, I trust, take into their candid consideration.

"I find on inquiry, since I was honoured with their lordships' communication, that when the Caledonia was dismantled:—

"1st. That her three working top-masts, having been in the ship for more than seven years, were so rubbed in the caps and otherwise worn, that they were not considered fit for further service.

"2nd. That no kind of defect was discovered arising out of the application of lightning conductors; that so far as the conductors were concerned, the masts might have been again used.

"We learn, therefore, from these facts, that the conductors remained perfect in the masts up to the time of the masts being considered no longer serviceable, and that since the plates of copper were still good, they might, consequently, be re-applied to other masts of the same dimensions; without any new expense except in labour.

"3rd. That the three spare top-masts, at sea in the ship for more than seven years, were returned into the store as serviceable top-masts, and might, if they had been permitted to remain in the same state in which they were returned, have been re-issued, either to the Caledonia or to another ship of her class, without the necessity of any alteration whatever. That for some reason not explained, the plates were taken out of the masts, and, of course, as a necessary consequence, the shallow groove in which they were inserted left exposed.

"As these spars were never intended to be used without the conductors, any reduction contingent upon their removal was a matter of choice, such removal being quite uncalled for.

"I would still, however, respectfully submit to their lordships, that even although the plates should be removed, a reduction of the spar is not abso-

lutely necessary; for if an oak batten was inserted in the grove, in place of the copper, and the whole planed off fair with the round of the mast, I am prepared to shew that the spar would be as serviceable as at first.

"Admitting, however, that the spar must be reduced, it is still not necessary to do more than pair off the small projection of the groove, (which is, after all, very little more than a quarter of an inch in depth,) the diminution of strength by this is really inconsiderable, and the mast might still be re-issued. The Spartiate's jib-boom, for example, was re-issued in this way, and, I believe, answered well.

"4th. That in the conversion of the top-mast to a brig's main-mast, the requisite reduction carried all round the spar *was not so great on account of the groove as was found necessary to bring the spar down to the required size.*

"Should it ever be requisite to convert a top-mast once fitted with my conductors to any other purpose, the necessary reduction is always much more than is contingent upon the groove for the lightning conductors.

"Their lordships will, I am sure, allow, that if after more than seven years, the wood was, on the removal of the copper plates, found so perfect as to admit of the mast being converted into so important a spar as a brig's main-mast, we have not much to complain of on account of the application of the conductor.

"I would in conclusion respectfully call their lordship's attention to the fact, that out of eleven ships fitted with the new conductors, few, I believe, now remain in commission, except the *Beagle*.* That although on being dismantled, their spars, with the conductors in them, remained perfect, and so far as the conductors were concerned, fit to be re-issued, yet, in several instances which have come to my knowledge, the conductors have been taken out of the masts, and the masts used for various purposes. I have no doubt the mast makers can explain why they have been led to do this in many cases, but why they have done so in others does not immediately appear; as no complaint has ever been made of the conductors so far as the masts were concerned, and that without any additional expense to the country the serviceable masts might still have been applied in the same way, and many ships been furnished with this protection from lightning."

"It is well known to their lordships that the *Beagle* was full *five* years on service, and that yet she has gone to sea with the same spars and conductors in them, on an equally long voyage, with the exception of new top-gallant masts."

"I cannot but respectfully bespeak their lordships' attention to these facts."

"And remain, Sir, &c. &c.,

WM. SNOW HARRIS."

"To Charles Wood, Esq., M.P., &c., &c."

XXVIII.—Mr. W. STURGEON's *Letter to W. SNOW HARRIS, Esq. F. R. S.*

Sir,—I hope you will acknowledge that I have given publicity to every part of your letter, that can possibly be useful either to yourself, or to the cause of your marine lightning conductors; to have published the other part of your letter, could have answered no laudable purpose whatever. I can have but very little to say in reply, as the opinions which I have already entertained, and which are already before the public, are not in the least affected by any facts which your letter contains. Your answers to the queries in my letter are

* That is to say, in which the conductors still remain perfect.

partly satisfactory and partly otherwise. Your explanation of the expansive effects of lightning are perfectly satisfactory, because you necessarily admit that lightning was the *primitive* cause, which admits of the correctness of all I have said respecting a *lateral discharge* of the *first kind*.*

Your explanation of the reasons which led you to proceed in the manner you did with the experiments at Plymouth, before the Navy Board, appear to me anything but satisfactory. That your conductor on board the *Louisa* cutter, was perfect enough to carry those electric charges which you transmitted through it, there can be no doubt whatever; but, that it shewed any peculiar advantage of action over other conductors I must still deny; although you say that, "*I shut my eyes to these plain deductions, and tell my readers that the experiments prove nothing peculiar to your system of conductors, &c.*" Now his sentence of yours, obviously implies a claim of some peculiar advantage of your system of conductors being demonstrated by those very experiments, and a censure upon me for not telling my readers that such was the case. Had I said anything otherwise than that which I did say respecting the character of the Plymouth experiments, I should have told my readers an untruth; and I think that not only *my* readers, but *your* readers also, will see pretty clearly, that I was perfectly correct, in stating that "those experiments are no more illustrative of the efficacy of Mr. Harris's system than of any other ever yet offered to public notice," when I point out to them your own words on this matter, which are the following. "Now I never asserted that any other conductor *would not* convey an electrical charge to the sea. My experiments were never *instituted* under such an impression."* I am sure that both of our readers will be much pleased to find that you have so ably set this matter at rest.

With respect to the "horn book"† work which you speak of, I have no means of knowing anything further than that which the character of your experiments indicates, which, to an electrician, would not appear very conclusive. And the reason you have given for employing gunpowder to show the effects of lightning on a ship's mast, are quite unsatisfactory.‡ Had you continued your illustrations on the small model, which for the first time, you now speak of, they would have been perfectly satisfactory. It is an old experiment and quite conclusive; but I must certainly still indulge in the opinion that your *gunpowder* experiment was not only quite out of place, but

* Fourth Memoir, paragraph 139, page 174, vol. iv. of these Annals.

† Fourth Memoir, page 166, vol. iv. of these Annals.

‡ Mr. Harris's letter, page 209. of this Number.

tended to give a false idea of the nature of the action by which masses of wood are cleft by flashes of lightning.

I am of opinion, also, that you are led into error even "under the new and very delicate circumstances" by which you "tried the experiment."* For if the detonating powder *did not* fire in the *interrupted* part of the circuit, by *your* experiment, that can be no very decisive reason why it should be so extremely obstinate in other hands. The "horn book" informs me that each branch of the conductor will carry a portion of the charge if sufficiently powerful, and the interruption in one branch of the conductor be only small.

Your view of my "lateral discharge" *at a distance of 50 feet* from the *direct discharge*, seems to have led you into some considerable error concerning *lateral* discharges in the body of a ship from direct discharges through my system of conductors in the rigging. I think that I have stated pretty clearly that this 50 feet, was 50 feet of metallic wire, (see page 175, vol. iv. of these Annals); and I never yet understood that there was a direct metallic communication between the outside of a ship and her powder magazine!!! or that the one was very near to the other: and I think you will admit that the distance of your conductors from the magazine is very trifling indeed, when compared with the distance between the latter and the outside of the vessel.

Moreover, the distribution of my conductors in the rigging is such that every flash of lightning which struck them, would be equally distributed amongst them before it arrived at the body of the ship: so that a small fractional part only, would be carried by any one of the lower branches: and as each branch conductor would carry an equal share, the forces on the two sides of the ship would be so completely balanced as to neutralize each others action on bodies placed directly between them, not only as regards *lateral discharges*, but also as regards the magnetic action of heavy flashes of lightning: for although an electric discharge traversing a single conductor, will magnetize a ferruginous body, a needle, for instance, placed within the sphere of its action, yet no discharge of electricity which passed equally on *both sides* of the needle would magnetize it: because one part of the electromagnetic action would counteract the other part of it, and they would mutually neutralize each others effects.

In regard to "Dr. Wollaston's judgement" on matters of philosophy, I shall always have a great veneration, and whatever degree of approbation he may have happened to confer upon your conductors, I should have found very little difficulty in making that philosopher sensible of the dangerous effects of

* Mr. Harris's letter, page 210 of this Number.

their electro-magnetic powers when traversed by heavy flashes of lightning. This is such a simple and common "horn-book" affair, yet such an important consideration in the disposition of marine lightning conductors, that its omission in the report of the committee is an *event* in British science, which leaves you and the *scientific councillors*, in no very enviable position. And although it may not have occurred to you before, that the situation of your conductors would give them a most dangerous influence on the chronometers and compasses of the ship, yet now that I have clearly pointed out the fact, it behoves you, at this critical period, to make known to the Admiralty that such is the case, in order that some means may be adopted to prevent those serious consequences which your system of conductors can hardly fail to produce.

I do not find that any other part of your letter requires my notice, and as I have met every other effort which you have made in favour of your conductors, in my former letters, without experiencing the slightest reason for altering my first statements, made in my fourth memoir, I necessarily conclude the discussion, under the same impressions as those with which I began. The errors into which you have occasionally fallen in those papers which you have published since the appearance of my fourth memoir, have certainly tended to rectify my former views of your mode of philosophical reasoning, which, I believe, is the only remuneration I need expect; unless, indeed, my exposure of the dangerous tendency of your lightning conductors may induce those in authority to pause, and re-investigate the whole subject, before any decisive steps may be taken for fitting out the British fleet with any lightning conductors whatever. And as I have some reason for supposing that such will be the case, I am still in hopes of experiencing the great satisfaction of having been instrumental in averting those personal and national calamities, which, in every probability would occur from the effects of lightning, were our fleet to be furnished with conductors such as you have proposed. And should I even be disappointed in that particular, it will always be a gratifying reflection that I have pointed out the means whereby those dangers might be averted, at an expense of little more than the first cost of the material; and without detaining any ship in harbour, or causing any other obstruction in the performance of any part of her duty, whatever may be the nature of her service, and on whatever station she may happen to be placed.

I have the honor to be,

Sir,

Your obedient Servant,

W. S. Harris, Esq.

W. STURGEON.

XXIX.—*On the cause of the change in colour which takes place in certain substances under the influence of Heat.***By C. S. SCHOENBEIN.**

It has not fallen within the power of man, up to the present time, to determine the relation which exists between the chemical nature of a body and its colour; it is probable that the determination of this difficulty presents one of the most difficult problems that philosophers and chemists will have to resolve. We know not why copper is red, gold, yellow; the cyanite of iron blue, and, in particular, we are completely uncertain whether the cause of the colour of a substance ought to be sought for in the nature of its molecules, or in the particular mode of their aggregation. But whatever be the obscurity which reigns in this point of view, and however great our ignorance on the true cause of the colourization of bodies, we know, notwithstanding, that the fact which determines the chemical nature of any substance is that which decides, before any thing, its relations with the light; and, in fact, there are a hundred cases in which we may conclude with certainty, that a chemical modification has taken place in a body after a modification of colour has been observed. But it is not only these luminous phenomena, with relation to colour, which are frequently modified by the effect of the chemical changes of the substance; those of this species of phenomena which may be referred to refraction, to reflection, to inflection, and to polarization, are under the same influence: and, in short, we may safely affirm, that in order to arrive at the establishment of the identity or the chemical difference of substances there exists no re-agent more sensitive than light.

Up to the present time, with our chemical means, we have only been able to determine amongst bodies those differences the most gross, and easily to be perceived; and, without doubt, also, for this same reason, we have admitted as identical with each other a great number of substances, which, upon examination, by the aid of re-agents the most delicate, will eventually demonstrate to us that there is no identity between those bodies. It is then very desirable that opticians should come in to the aid of their chemical brethren, and by furnishing them with such instruments as are necessary, enable them to determine, in a manner at once easy and certain, the slightest qualifying modification which takes place in any which may be subjected to their inquiry. When once the research into the chemical nature of substances, by optical means, shall come into general use, I am persuaded that our

knowledge in this respect will be rapidly extended, and we shall acquire more correct ideas than those we possess at present on the intimate nature of substances. The researches of Newton on the power of bodies to refract light, and the researches, still more recent, of the celebrated Biot, have already placed in a striking point of view the great importance it may be to the chemist to have a knowledge of the optical character of bodies.

I have no other end in view, in publishing this work, than that of calling the attention of philosophers and chemists on the importance of the momentary and sudden changes of colour which divers substances undergo under the influence of heat.

In comparing the chemical nature of substances which present this phenomenon in these indicated circumstances, it ought to excite our surprise, above all, that it is only observable in compound bodies. Sulphur, phosphorous, and perhaps, also, selenium, which are considered as simple substances, form exceptions; but the property which these bodies possess of taking various colours in different circumstances, ought, perhaps, to be sufficient for us to presume that they are compound, more especially if we consider that sulphur, and in all likelihood also, the two other substances are.

The number of compound substances which are possessed of the properties of which we are now speaking is very considerable, and it would be too tedious to name them. Amongst those which are solid, I content myself with naming the red oxide of mercury, which, upon being heated, takes a brownish black colour. The yellow basique nitrate of mercury, which in the same circumstances assumes a red colour; the red iodure of mercury, which becomes yellow at an elevated temperature; the citron yellow coloured chromate of potash, which takes an orange colour on being affected by strong heat. The liquids, with some exception, change colour in general when they are heated. A solution of muriate of cobalt, for example, which when cold is of a yellow brown colour, becomes blue on being heated; an acid solution of nitrate of iron, which at the ordinary temperature is completely colourless, becomes a reddish yellow when heated. Nitrous acid, (colourless at 20°) becomes yellow, and even a red brown on being exposed to heat: the colourless combinations of this acid with nitric acid, sulphuric acid, phosphoric acid, &c., becomes equally yellow in the same circumstances. Among the compound gases I know of none of which the colour is sensibly changed by the effect of heat except it is the nitrous acid gas, whose colour is, as is well known, of a deeper hue

at a high temperature. But it is very likely that a more profound examination will shew that other aëriform bodies also change colour with their temperature.

The question in point now is to resolve, to what cause the phenomena in question may be attributed; whether it is to mechanical circumstances, or to chemical changes. Up to the present time we have always sought to explain it, by vaguely admitting that heat produces a certain modification in the arrangement of the intimate molecules of a body from whence proceeds a change of colour. This hypothesis may possibly be exact, generally speaking, but it is so vague and indeterminate that it leaves, in a complete uncertainty the question of ascertaining whether heat only changes, the relative position of the compound molecules, or whether the simple heterogeneous atoms combine among themselves under the influence of an elevated temperature, in other affinities than when the body is not heated. Some recent researches of Mitscherlich, Rose, and other chemists, have demonstrated that certain salts undergo an essential modification, one might almost say chemical, though this was not a decomposition in the ordinary sense of the word. Thus the arroganite, heated to a full red heat, is transformed into calcareous spath; the red pyrimidal iodure of mercury into the prismatic yellow iodure of mercury, without the observer being able to perceive any modification in the composition of these two bodies. Other examples of the same nature might easily be cited.

An important circumstance, to which I shall endeavour at the present time to draw your attention, is this, that the red iodure of mercury, on being transformed into yellow iodure by the action of heat, persists some time still, it is true, in its new estate after being cooled, but does not fail, nevertheless, to take its primitive state without the sensible intervention of any exterior action, though mechanical causes, such as a sudden shock, singularly hasten its return to the normal state. The arroganite once transformed into calcareous spath, undergoes no further change. There is not the least doubt that in the case whence arises the question, that heat only produces in the chemical nature of bodies that modification which chemists call *isomeric*. They form new substances, which are distinguished in particular from those from which they proceed by a peculiar form, by their specific gravity, by their hardness, and by their action on light, and, in all likelihood, by other physical properties.

What is it now which takes place in those substances which thus change colour with the temperature? Does this change indicate different chemical combinations among the constituent

elements, and ought it to be regarded as a proof that the mass resting identical, the same element can form a series of isomeric combinations, of which each in particular is produced by a determined temperature? The change which takes place in the red iodure of mercury appears to me to be of particular importance in the answer to these questions; on one hand, because the phenomena which this body presents approach in appearance those of bodies which the act of cooling causes to return to their former state, (the elements of this iodure not persisting, in fact, in their new combination); on the other hand, because the body approaches also to carbonate of chalk, the iodure not taking immediately its primitive state when the cause which modifies it has ceased to act. Under the relation of the variability of its molecular composition, this iodure places itself then between the calcareous carbonate and the combination in which the change of temperature, and the modification in the chemical constitution, only remains as much in one as the other.

Let us now seek a solution of the proposed question, at first in that which concerns the oxide of mercury, which presents a change of colour so remarkable. The mode of combination of oxygen with mercury, under the relation of intimacy, ought not to be the same at elevated degrees of temperature as at inferior degrees: it is already acknowledged, that at a certain temperature these two substances separate one from the other, and it will be easily admitted, that as the oxygen holds much more feebly to the mercury, that the oxide is most heated. Now, a difference in the intimacy with which the same elements are combined constitutes already, according to my ideas, a difference either qualifying or chemical. After that, the oxide of mercury in a heated state, is chemically different from the cold oxide, and there exists between the two an isomeric relation. To speak more properly, all the chemical combinations which are of different temperatures, are, it is true, in the same case, but particularly those which heat alone decomposes. It seems possible to me, however, that many compound bodies undergo, in their intimate nature, by the effect of heat, modifications which may have, it is true, a distant reason in a change of relations of affinity, but which are owing, above all, to a momentary derangement of the constituent elements, out of their normal position taken at the ordinary temperature. It is, in effect, a singular fact, that many compound bodies take, when they are heated, a colour which characterises another degree of combination of the same elements. The following examples will serve to make the case in question better understood.

At an elevated temperature, the oxide of mercury takes the colour of the *oxidule* of the same metal; the deuto-sulphuret of mercury, the colour of proto-sulphuret; the proto-chromate of potash that of double salt; the colourless solution of nitrate acid of iron that of the solution of a nitrate basique of that metal; the solution yellow and neuter of muriate of cobalt, that of the acid solution of the same metal, &c. Now though this shadowing change may not be observed in each of the substances which change colour with the temperature, the cases where it presents itself are however so very numerous, that we are not at liberty to consider this phenomenon as simply the effect of chance, nor to suppose that the changes in colour of these substances are owing to the formation of a new combination: as for example when the deutoxide of mercury is heated it is transformed into protoxide, the neutral salt of chrome into acid salt, the neuter solution of muriate of cobalt into an acid solution, &c. But as, in the examples here cited, the heat does not separate oxygen, nor potash, &c. it is necessary to admit that these substances are found in an intimately mixed state in the bodies which we have submitted to the action of heat, or else, that the new combination possesses still so great an adhesive force for the substance which has been insulated, that it cannot be separated from it. It is possible, also, for example, that a molecule of mercury may be found, during the process of heating, nearer to one of the particles of oxygen enclosed within an atom of oxide than another, and that this second particle remains attached to the oxidule by the effect of a species of affinity, and is thus prevented from disengaging itself under the form of gas. We may suppose again, that there is established, sometimes between the two elements of a combination exposed to the action of the heat such a relation that they are, it is true, completely separated one from the other under the chemical relation, but that they are still retained together by the effect of an attraction, similar to that which Faraday believes to be exercised by platinum on oxygen.

It is the skilful Kiehmeyer, if I mistake not, who has advanced, now a long time since, the idea that each particular temperature has its own chemical power. Without wishing to take this assertion for granted, I believe, however, that it is true in general, and that a proof is furnished of its correctness in the change of colour produced by heat in compound bodies. As I have already made the remark, that chemists have not, during a long time, taken account only of the most glaring differences of bodies, and have regarded as identical those substances which have given as the result of analysis

the same elements in the same proportions. The discovery of isomery, and of *dimorphie*, with which it is intimately connected, has elicited the fact, that the equality of elements, and the proportions in which they are combined, is not a certain criterion of the identity of chemical substances, and that, in this case, it is quite possible that there may exist great differences in the physical and chemical properties of bodies. However, let this point be once established, and that in particular it has been demonstrated, that by the aid of heat we can accomplish not only the decomposition of substances, but, likewise, isomeric transformations, we may hope that chemists will direct their attention to the qualifying modifications less apparent, and particularly on the transient modifications which these bodies undergo under the influence of heat. Researches of this description would not fail to extend the actual limits of chemistry, and give us more correct ideas on the different modes of combination of elementary substances, as also, to spread the light of day on the relation which exists between the molecular constitution of a body and its physical and chemical properties.

In order to find some experimental proofs in favour of the opinion which I have advanced in relation to the cause of the change of colour on many substances, I have had recourse to the galvanometer. It is a fact, recognised by most philosophers, that every chemical modification, formation, or decomposition, of a compound body, has the effect of destroying the electrical equilibrium of substances which act upon each other. Upon this principle, if the change of colour, now in question, is to be attributed to any chemical modification whatever in those substances in which it is observed, we ought also to see established a voltaic current, and to be able to demonstrate the existence thereof, in favourable circumstances, by means of the multiplicatier. Now, to speak of solid substances whose colour changes with the temperature, they are, unfortunately, such bad conductors of the voltaic current that they do not leave the least possibility of availing ourselves of the use of the galvanometer. It is not the same, however, in liquid substances; but I have made use of them to make a series of experiments, of the results of which I have hereafter spoken.

We know that an acid solution of chlorure of cobalt, a little concentrated, is blue, but that it becomes yellow by the addition of a small quantity of water. If this yellow liquid be heated, it retakes its blue colour, and this colour becomes deeper as the colour of the liquid is more elevated. The chemists explain this passage from blue to yellow by supposing the

water to change to acid salt a part of its acid, and that thus the yellow solution encloses another combination than that which is found in the blue solution. As the yellow liquid again becomes blue by a new addition of hydro-chloric acid, and as heat alone produces also this change of colour, we can easily suppose that at a more elevated temperature the yellow neutral solution of cobalt is transformed into the acid blue combination; or, what is precisely the same thing, that the acid carried off by the water to acid salt separates itself a second time from the water by the effect of heat, and forms anew with the neuter combination of acid chlorure. But if chemical modifications of this species do really take place, the electrical equilibrium in the liquid in question ought also, in consequence of what I have already advanced, to be destroyed in these circumstances. If now we put the liquid into a tube in the form of U, and a platinum wire in each branch of the tube, and now heat the column of liquid in one of the branches, until it becomes blue, and then bring the free extremities, of the platinum wires into communication with a delicate galvanometer, there is a current established which travels from the cold column of liquid to the heated one, and we find the force of this current to be greatest, when the difference of temperature between the two branches is most considerable. In my experiments the deviation was about 70° when the liquid was near the point of boiling, that is to say, when the colour was the deepest. I need scarcely add that the needle returned to zero, as soon as the two divisions of the liquid had arrived at the same degree of temperature; that is to say, as soon as the blue colour of the one had again taken the place of the yellow.

Such was the exactness also of the manner in which the galvanometer carried itself in the solution of nitrate acid of iron, which is colourless at the ordinary temperature, and which takes a yellow colour on being heated. In the same circumstances as those already given, I have obtained a current which travelled equally from the cold portion of the liquid to the heated one, and which made the needle deviate about 40° . The results have been similar to these too, when, in the place of the liquids of which I have spoken, I have made use of a solution of acid sulphate of iron, or of liquid combinations of nitrous acid with other acids, such as sulphuric acid, phosphoric acid, nitric acid, &c.

In truth, it seems at first that these observed currents are of a thermo-electric nature; that is to say, that they are the effect of the difference of temperature of the two liquids, or of the two platinum wires. M. Becquerel says, in his "*Traité de l'Electricité*," that when the two extremities of the plati-

num wire of the galvanometer are plunged into nitric acid, and that in these circumstances, the electrical equilibrium is maintained, this equilibrium will be destroyed, if we come to draw one of the extremities out of the liquid, heat it, and plunge it in anew, and that then a current will be developed which travels from the cold extremity to the hot. The French philosopher considers this current as being of a thermo-electric nature. But if this opinion have any foundation, we should be able to obtain similar currents with all liquids which are good conductors. Now my researches on this point have convinced me to the contrary. Sulphuric acid, perfectly pure, alone, or with water in different proportions, pure hydrochloric acid, the dissolutions of potash, sulphate of potash, carbonate and phosphate of alkali, sulphate of zinc, corrosive sublimate, and many other salts, have been put successively into bent tubes; the part of the liquid enclosed in one of the branches only has been heated, and when I have established the communication with the galvanometer, by means of the platinum wires, I have not obtained even the most feeble current. The absence of the current in this last experiment, seems to bring clearly in view the inaccuracy of the explanation given by M. Becquerel, and at the same time the great likelihood, or else the certainty, that the destruction of the electrical equilibrium, when it coincides with the change of colour in a liquid, is not immediately owing either to the difference of temperature of the two wires, or to that of the two portions of liquid contained in the branches of the tube, and communicating with each other, but to transient chemical modifications, which have been produced by heat in one of these portions.

It is scarcely necessary to remark expressly, that it is possible also to have liquids whose colour does not change, in whatever way they are submitted to the action of heat, and in which notwithstanding, transient chemical changes, take place, for the qualitative changes of a body are not always necessarily accompanied by a change of colour. Those liquids which are found in this position, should also, by consequence, be in a state to produce a current when they are unequally heated. Now the result of my experiment proves that the dissolutions of many nitrates of mercury possess in a very high degree the property in question, when they are submitted to an unequal heat, and it is known that the solutions of this species are colourless at very different temperatures.

Let us now suppose that the preceding remarks are perfectly exact; the result would be that a galvanometer would

offer to the observer an instrument, which would put him in a position where he could demonstrate the existence of chemical actions, in which no reaction is announced, and where up to the present time no modification was believed to be operating in the chemical constitution of the substance under observation. I have already named, besides the galvanometer, the chemical microscope, and I believe that the facts which I have just described are sufficient to justify this denomination. It will be by consequence very desirable, that those chemists who are devoted to scientific research, will make use of this precious instrument more frequently than they have hitherto done, and that they will make above all an attentive examination of all the important chemical combinations, which serve as conductors of the current, in order to know the influence which they exercise on the galvanometer when they are unequally heated.

Permit me, in terminating this work, to express some ideas destined to make comprehensible the importance which isometry will probably sooner or later exert, for that part of geology which is united to chemistry. In considering under a chemical point of view the parts which constitute the crust of the earth, we ought to be surprised to see certain elements predominate over others in the rocks of certain geological formations. I do not wish to recall here the enormous masses of calcareous carbonate, which are encountered in those which are called sedimentary earths. On the other side it is not rare to find in the same formation chemical productions extremely different, placed one by the side of the other, and, what is very remarkable, in such a manner sometimes that the one passes the other by degrees almost insensible, as is seen for example in the calcareous carbonate and the dolomite. These transitions take place sometimes under circumstances which cause us to think of the transformation of one of these substances in the other. And, in effect this idea has been suggested at an anterior epoch, but has ordinarily been repulsed as a product of the imagination of the alchemist, and has been declared completely inadmissible.

Under a chemical point of view, we ought, it is true, to suppose that since our earth exists, the same elements of which we have knowledge at the present day have always existed, and that all the diverse geological formations, inasmuch as they relate to chemical actions, have been owing to the affinity of one of these elements for the other. As to the transformation of one substance into another, it is a fact we are not able to admit; and then say, how, under the relation of quantity, the elements are united in such a manner as to be able to

form precisely compound combinations after the chemical proportions, and how these substances, which have power to combine among themselves, have so happily met: the chemists' do not believe themselves bound to give in this point of view any plausible explanation, and they put this fact to the number of those on which actual science does not possess sufficient elements.

Further, the singular fact, that certain substances, accompany each other, or always avoid each other, and that these substances, are then found as often to be of bodies which offer a passable resemblance in their chemical character, such as, amongst which we find together, the chlorine, brome, and iodine; sulphur and selenium; platinum, iridium, palladium, baryte, and strontian; potash and alkali; this fact ought to be considered by the chemist of our days as a pure hazard, two elements being always separated in his eyes by an abyss quite impossible to overcome.

In the opinion of many philosophers, there may have been a time in which all the constituent elements of our planet may have existed in a state of insulation. But this hypothesis implies here, that the compound bodies with which we meet at the present day, have been formed one day by means of synthesis. We can, according to my ideas, make good several arguments which are not favourable to this point of view, and which permit the supposition that many chemico-geological products have been found by another way than that of composition, by means of elements which we can at the present day separate. If the substances which we regard as elementary, were once found in a state of complete insulation, and they may have been at the same time submitted, as at our day, to the law of gravity, they would have been obliged, it seems, to arrange themselves the one on the other, according to their specific gravity. But as it is easily comprehended, would alone have sufficed to hinder the combinations of several of those elements which are now found united. In truth, when the chemist pretends that in the primitive times, these elements, disposed one on the other, in beds or layers nearly concentric, well mixed together by the effect of an unknown cause, and which will come suddenly into activity (an hypothesis which is accorded to it, as we accord to astronomy that of an impulsion, when there is need to explain the curvilinear movement of the planets), this hypothesis will make comprehensible the existence of many geological products; but at all times, a great number of geologico-chemic facts will remain enigmatical to us, and even inexplicable.

But if many substances which we consider as compound,
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have not been formed by the way of ordinary synthesis, it is still a question, at least to admit, for the sake of convenience, that they have been created such as they are at the present day, or that they have always existed in the same state, it is yet a question, what origin have they had. As for myself, in acknowledging all that we cannot yet answer to this question, no more than others, concerning the origin of mineralogical products, I think that isomery will aid us as we advance to resolve a great number of chemico-geological problems. As this new branch of chemistry becomes developed sufficiently, for the acknowledgment as isomeries, those substances which we have been forced to regard up to the present day, as elements, and we shall the light of day spread itself on many subjects which are yet covered in a profound obscurity.

It is a true principle, and let it for that be often repeated, that nature attains by the most simple means, the greatest and the most varied ends. How complicated and how grand are the effects produced by gravity, the action of which at the same time obeys a law so simple! If then we suppose that the numerous different substances which constitute our earth, are the product of a small number of elementary substances, united amongst themselves in a manner the most varied in their proportions, and in the mode of their combination, we have there an hypothesis which we are authorized to make by analogies, and which will scarcely leave us accusable of wandering in reveries or metaphysics. Let us represent the small number of substances which we admit hypothetically as elements, submitted to the influence of very different temperatures, of voltaic currents of different energies of various forces of pressure, &c., we may conceive how in these circumstances so different, the bodies the most different have power to be formed by means of a small number of elements, these bodies in particular, which we use for decomposition, relatively feeble, do not permit us to resolve into their elements. We know already some facts, which give us authority to presume, that some substances which are now regarded in chemistry as simple bodies and which ought as such, to be invariable in their essential properties, would evince very considerable modification when they are exposed to certain influences and in particular to those of electrical currents and to heat. It has long been known that sulphur can assume very different forms, and that, suddenly cooled after having been heated it passes to a state of coherence essentially different from that which it previously possessed. Phosphorous and selenium present similar phenomena. In my electrochemical researches, I have myself recently obtained results,

which demonstrate that iron, which is regarded as elementary, can, under the relation chemical and under the relation physical, take such modifications, that, in its new estate one ought to regard it in some manner as quite another metal. From the state of a body very oxidable, it is transformed into a substance neuter with regard to oxygen; a metal eminently electro-positive, it becomes a metal electro-negative. Similar modifications have already been observed in some other very oxidable metals. Though these modifications are it is true but transient, and that up to the present time no means have been found to render them permanent, it does not yet follow that, by, example, this result will not absolutely be obtained for iron. It results from this, that the above modifications, are a proof that many bodies which are called elementary, do not bear the character of absolute invariability, in that which concerns the properties which are ordinarily regarded as essential.

In like manner, as the chemist ought to furnish to the geologist his aid in extending that science, so ought the geologist, in his turn, to lend his aid to the chemist. Like light, are not the geological explorations spread on the history of organized beings, and what discoveries may we not hope to make in the field of zoology, by the researches even which are made in our days, on the nature of animals which peopled the primitive world?

We may be well permitted to suppose that the formation of the inorganic bodies of our earth has taken place after determined laws, as much even as that of organised beings which have perished; that in other periods of the history of our planet, there have been epochs of chemical formations, as there have been periods of organic creation: and is it not impossible that both the one and the other have been in a certain mutual dependance, and that one of these classes of phenomena has had in the other the cause of its existence? Now, if, in the actual moment, geologists were to make a strong effort to direct all their attention on the organic remains of primitive times, and, by a self-compulsory effort, construct, with the monuments of the first ages, a basis for the history of our globe; if, further, we should acknowledge that in the course of the last twenty years the zeal and the perpicuity of geologists devoted to this part of the zoology and botany which belongs to their science, have obtained the most extraordinary results in this domain, and have arrived at the solution of problems the most difficult, it ought not to be a case for doubt, that the chemical side of geology has not at least attracted that regard which it merits. But as it is certain that essential modifications, which our globe has suffered in anterior

epochs, have been produced by chemical forces, it will result therefrom, that the geologist must necessarily study the matter of the globe under a point of view purely chemical.

In order to arrive in the field of research at results which have some value in science, we ought to take the same ways by which the geognostic zoologists have acquired the knowledge which they possess on the organic life of the primitive world. We ought to study with the greatest degree of care each particular geognostic product; we ought to determine also, as exactly as it is possible, the mutual relations of these products in their nature, both chemical and physical, and in their chronological succession; and, at the same time, to make the most scrupulous comparison between the product which we obtain by the aid of chemical forces yet active at the present day, and the inorganic bodies of the primitive world. In a word, we ought to commence by creating a geo-chemic comparison before we can possibly have a true geology, before the mystery of the creation of our world can be discovered, and the masses which compose it. But in order to arrive at this elevated and truly gigantic end, which is proposed to science, it is necessary not only to take advantage of all the men who possess not only all the knowledge which philosophy and chemistry is able to furnish, but also who are endowed with that rare faculty of grouping and collecting, under different, general points of view, those masses of particular facts, and to discover a relation and connexion between phenomena altogether strangers in appearance. It is necessary that there arise a man who will be to geological chemistry that which Cuvier has been to the anatomy of the animal fossil kingdom, and what Newton has been to astronomers.

XXX.—*On Electro-type from Engraved Copperplates.*

By SAMUEL CARTWRIGHT, Esq.

Having been favoured by Samuel Cartwright, Esq., of Preston, with an excellent specimen of printing on address cards, from an electro-type plate which that gentleman had made, I immediately wrote to him requesting that he would favor me with the plate in order that I might be enabled to place specimens of printing, from it, before the readers of these Annals. Mr. Cartwright finding that he could not comply with this request, in consequence of the plate being presented to the Lancaster scientific exhibition, immediately proceeded to prepare another, which, as soon as ready, was very kindly presented to me by that gentleman, who waited on me at this Institution for that purpose and, in the most

liberal manner, communicated to me all the particulars of the process, which is the following.

To the back side of the engraved plate intended to be copied, is soldered one end of a copper wire; and to the other end of that wire is soldered the zinc plate of the operating voltaic battery, which consists of a single pair of metals placed in a porcelain jar, having the copper side in a solution of sulphate of copper, and the zinc side in water, or in salt and water. To the farther end of the conducting wire belonging to the copper side of the battery is soldered another copper plate, about the size of the engraved one. When the battery is prepared, the two copper plates at the farther extremities of its wires, are to be placed in a strong solution of sulphate of copper, the engraved one with its face upwards, and the other directly over it, but not to be in contact with each other. With this arrangement, the cuperous solution becomes decomposed, and the liberated copper is precipitated on the face of the engraved plate: whilst the copper plate of copper in that vessel, suffers dissolution, and feeds the solution with fresh portions of copper.

At the end of about three days the newly formed plate will be thick enough to remove from the engraved one, and the impression will be a very faithful copy of the original one, having the letters and other characters in *relief*, similar to those on a card printed from the engraving.

Since Mr. Cartwright's visit to this institution, Dr. Goodwin, of this town, has taken a very good electro-type plate from the engraved copper-plate from which his address cards are printed; and several other gentlemen are now preparing electro-type plates by similar means.

From my own experience, I find that the dissolution of the copper-plate, which is connected with the copper side of the battery, is very rapid, and when thin, if care be not taken, small fragments from it will fall down on the lower plate, which, if not removed, might injure the process. To prevent any accident of this kind, I put the upper plate in a muslin bag, which prevents the fall of any fragment. If, however, the plate be sufficiently thick to outlast the process, there will be no need of this precaution. The face of the electro-type plate is as smooth as the original, and every scratch, however minute, will be faithfully transferred.

WILLIAM STURGEON.

Royal Victoria Gallery, for the
encouragement of Practical
Science, Manchester.

P. S.—On my requesting to print from Mr. Cartwright's electro-type plate, I received the following letter.

Dear Sir,—I have no objection to your publishing, as you desire, the Electro-type I made for you ; but must request that your readers may at the same time, be informed that I consider it very inferior to what a more practised person would produce.—It is only the second I have made to print from, and before attempting this process I never constructed or used a galvanic Battery.—My reason for sending it to you was, as I have before stated, to shew that the process may be performed by any person who will take the trouble of reading the accounts of it published in you Annals of Electricity, and other periodicals.

The plate is just as it came from the Battery, no graver, burnisher, or even charcoal has touched it, except in drawing the line under my name, to distinguish its impressions from those of the original plate.

Yours truly,

SAMUEL CARTWRIGHT.

Preston, 21st Aug. 1840.

The next business is to proceed with the new electro-type plate, in the same manner as had previously been done with the engraved one ; and a *fac-simile* of the latter will be obtained. The process should be continued four or five days to give sufficient thickness to the plate to be printed from.

In order that the electro-type may leave the original plate without injury, Mr. Cartwright covers the face of the latter with bees' wax, and whilst warm wipes of the greater part, leaving only a thin film. The back part of the original plate and its conducting wire are covered with sealing-wax varnish, to prevent unnecessary deposition of copper on those parts.

In the electro-type plate will be seen two specimens, one from the electro-type, the other from the original plate.

W. S.

* When electro-type copies of medallions are to be formed in casts of plaster or other porous materials, Mr. Spencer gives the following ingenious mode of giving those matrices the necessary surfaces.

The porous matrix is to be dipped into a weak solution of nitrate of silver, a portion of which becomes absorbed. It is next exposed to the fumes of a warm alcoholic solution of phosphorus, and the absorbed nitrate of silver becomes decomposed.

Mr. Parry, an ingenious gentleman of this town, has lately obtained good electro-type impressions from the green leaves of trees, by properly placing those leaves so as to receive the deposited copper by voltaic copper.—W. S.

XXXI.—MISCELLANEOUS ARTICLE.

*On the Application of Electro-Magnetism as a Motive Power ; in a Letter from Prof. P. FORBES, of Aberdeen, to MICHAEL FARADAY, D. C. L., &c. &c.**

King's College, Aberdeen, Oct. 7, 1839.

My dear Sir,—Having seen a notice from Mr. Jacobi sent by you to the London and Edinburgh Philosophical Magazine,† regarding the success of his experiments on the production of a moving power by electro-magnetism, I am sure it will give you pleasure to know that a countryman of our own, Mr. Robert Davidson, of this place, has been eminently successful in his labors in the same field of discovery. For in the first place, he has an arrangement by which with only two electro-magnets and less than one square foot of zinc surface (the negative metal being copper) a lathe is driven with such velocity as to be capable of turning small articles. Secondly, he has another arrangement, by which, with the same small extent of galvanic power, a small carriage is driven on which two persons were carried along a very coarse wooden floor of a room. And he has a third arrangement, not yet completed, by which, from the imperfect experiments he has made he expects to gain very considerably more force from the same extent of galvanic power than from either of the other two.

The first of these two arrangements were seen in operation by Dr. Fleming, Professor of Natural Philosophy in this University, and myself, some days ago ; and there remains no doubt on our minds that Mr. Davidson's arrangements will, when finished, be found available as a highly useful, efficient, and exceedingly simple moving power. He has been busily employed for the last two years in his attempts to perfect his machines, during all which time I have been acquainted with his progress, and can bear testimony to the great ingenuity he has shewn in overcoming the numberless difficulties he has had to encounter. So far as I know he was the first who employed the electro-magnetic power in producing motion by simply suspending the magnetism without a change of the poles. This he accomplished about two years ago. About the same time he also constructed galvanic batteries on Professor Daniell's plan, by substituting a particular sort of

* Communicated by Dr. Faraday.

† See L. & E. Philos. Mag. for Sept., p. 164.

canvas instead of gut, which substitution answers perfectly, is very durable, and can be made of any form or size. And lastly, he has ascertained the kind of iron, and the mode of working it into the best state for producing the strongest magnets with certainty.

The first two machines, seen in operation by Dr. Flemming and myself, are exceedingly simple, without indeed the least complexity, and therefore easily manageable, and not liable to derangement. They also take up very little room. As yet the extent of power of which they are capable has not been at all ascertained, as the size of battery employed is so trifling and the magnets so few: but from what can be judged by what is already done, it seems to be probable that a very great power, in no degree inferior to that of steam, but much more manageable, much less expensive, and occupying greatly less space, if the coals be taken into account, may be obtained.

In short, the inventions of Mr. Davidson seem to be so interesting to rail-road proprietors in particular, that it would be much for their interest to take up the the subject, and be at the expense of making the experiments necessary to bring this power into operation on the great scale, which indeed would be very trifling to a company, while it is very serious for an individual by no means rich, and who has already expended so much of his time and money for the mere desire of perfecting machines which he expected would be so beneficial to his country and to mankind. For it deserves to be mentioned that he has made no secret of his operations, but has shewn and explained all that he has done to every one who wished it. His motives have been quite disinterested, and I shall deem it a reproach to our country and countrymen if he be allowed to languish in obscurity, and not have an opportunity afforded him of perfecting his inventions and bringing them into operation, when the promise to be productive of such incalculable advantages.

L. & E. Philos. Mag.

Fig. 2.

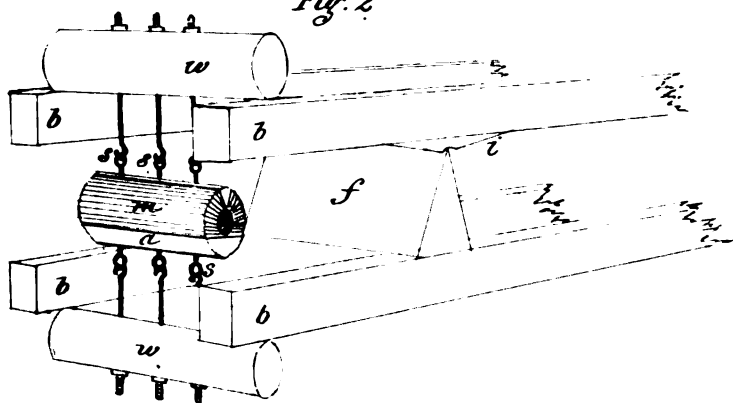


Fig. 1.

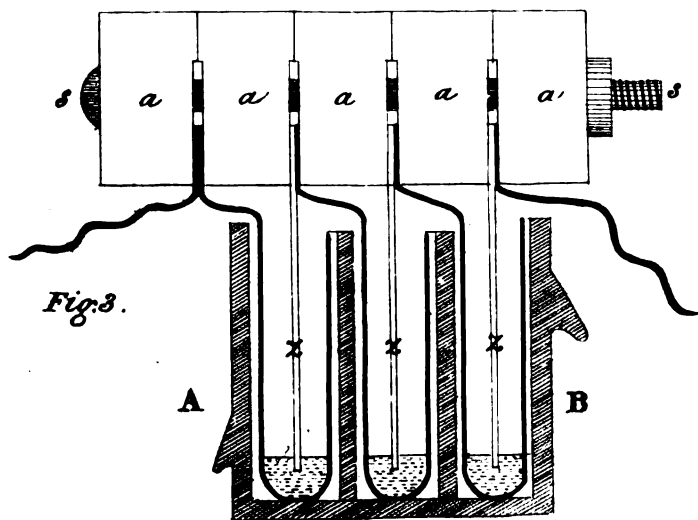
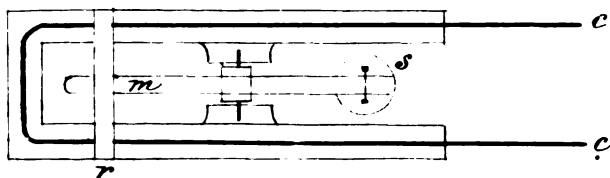


Fig. 3.



Printed from

AN ELECTROTYPE.

— Prepared for & Presented to —

W^h STURGEON ESQ^{re}

by Sam Cartwright

Boston Aug. 1840.

Printed from

AN ELECTROTYPE.

— Prepared for & Presented to —

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THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;
AND
Guardian of Experimental Science.

OCTOBER, 1840.

XXXII.—PROFESSOR MARIANINI on a Leyden Jar that becomes charged by means of simple contact.

From the Memorie di Fisica sperimentale, Modena 1838.

*Mem. vi. p. 88.**

8. Convinced as I am of the truth of the contact theory of electricity, I never for an instant doubted that a Leyden jar, whose two coatings consisted of two different metals, would become charged every time that these coatings themselves were brought into metallic contact with each other. But it was no easy thing to say before-hand whether or not the charge would be great enough to give us indications of its presence by the electrometer, or other instruments. For the plates of a condenser give indications of tension as we lessen their capacity very considerably by separating the plates from each other, whereas the coatings of a jar remain always at the same distance. Added to which the capacity of the jar, supposing the surfaces of the two equal, is much lower than that of the condenser, inasmuch as the intervening stratum is very much thicker in the former than in the latter. And it was on

* Translated by W. G. Lottsom, Esq.

that account that I commenced with several experiments touching the effect of charges of the Leyden jar brought about either by the contact of dissimilar metals, or by a pair of plates, as already detailed in § 5 and § 6, and from which it was clear to me that I should be able to obtain the arrangement I had in view with an apparatus of moderate dimensions.

A large glass bottle or jar nearly of a spherical shape, and the thickness of which, as shown by examining it in different spots, did not anywhere exceed a millimeter, was, with the exception of the edge and a zone of about three centimeters wide adjoining thereto, coated over externally with zinc foil. The inside of the jar, corresponding to the covered portion outside, was coated with silver leaf. The edge and half of the above-mentioned zone was covered, both inside and outside, with a coating of fused sealing wax. From the bottom of the jar there rises a stem or rod of silver, having at the upper end a longish slit furnished with a stout ring, which serves for securing a flexible silver wire of about five decimeters long.

The jar is surrounded by a broad ring of zinc on which it rests, and this ring is supported by three glass rods covered with sealing wax and fastened into a wooden stand, which is likewise covered throughout with a similar coat of insulating materials. To this hoop of zinc there is attached a small binding screw of the same metal, used for securing one end of a wire or strips likewise of zinc. Each of the coatings contains a surface of about 26 square decimeters.

Two small discs, one of zinc and the other of silver, are ground flat on one side, while from the other there rises a small screw for fastening each on to a little rod of glass which is provided with a female screw formed of the same metal as the disc that is to be screwed on to it. The extremity of the wire connected with the internal coating of the jar is attached to the silver disc by the corresponding screw, while the thin strip of zinc, communicating with the external coating, is in a similar manner connected with the other screw.

By means of these screws there may be likewise adapted to each of the two discs one or two ribbons or tongues of metal, and these may either be the same as those of which the discs are made or not.

By the side of the jar, and insulated in a similar manner, I placed a silver leaf electrometer furnished with a condensor, whose lower plate is of silver, but whose upper one is of zinc.*

* Fig 1 of Plate (V.) represents the apparatus in question.

A B C, is the glass jar or bottle.

A a C c, represents the zone coated with melted sealing wax within and without.

D B E, the portion of the jar coated externally with zinc foil, and internally with silver leaf.

F, the upper end of the stem or rod of silver, fixed to the internal coating at the bottom of the jar.

G H, a ring or hoop of zinc surrounding the jar.

GI, I, HI, three supports of glass coated with sealing wax, supporting the

9. On examining the jar with this instrument by bringing the zinc disc, or one of the tongues connected with it, into contact with the zinc plate of the condensor, while the internal coating was brought into contact with the silver plate of the condensor by means of the silver disc, I did not meet with any trace whatever of electrical tension. But on repeating this examination after having for an instant touched the zinc coating with the silver disc, I found the tension amounted to about nine degrees.

When the two coatings were brought into contact with a moist card by means of the tongues above-mentioned, no charge could be communicated to the condensor. But on the zinc coating being again touched with the silver wire it became suddenly charged, as before, to nine degrees.

10. Having discharged the condensor and the electrometer of these nine degrees of tension, I placed it a second time in connection with the two coatings of the jar, and without the latter having been re-charged, and yet the electrometer showed a tension of five degrees. And I thus re-charged the condensor a third, a fourth, and a fifth time, the tension decreasing at every trial.

On one occasion I repeated this experiment under more favourable circumstances, (it was on the 31st of October, 1838) and the twentieth time of discharging the jar as described I had a tension of two degrees. The experiment was interrupted at this stage owing to a copper wire falling on the two discs and thus re-charging the jar.

The jar in question was likewise charged by slow degrees also in the following manner:—Taking hold of the insulating handles carrying the discs connected with the wires proceeding from the two coatings, I immersed the extremity of the silver tongue in a cup of salt and water, and then having withdrawn it I immersed the extremity of the zinc tongue, and having in like manner withdrawn this, I repeated the immersion of the silver one, and then again of the zinc one, and did so ten times. I then made the discharge through a prepared frog, on which a trifling contraction ensued. On repeating the experiment, the jar, however, being discharged little by little by thirty of these alternate connections, the residual

above ring, and with it the vessel. They are let into the stand L.

F M, a silver wire proceeding from the rod F, and connected at the other end with the silver disc M.

N O, a strip of zinc fastened at N by a binding-screw of the same metal to the hoop G H, and inserted at the other end into the zinc disc O.

T, T, insulating glass handles to which the discs are attached.

P, Q, two tongues of silver connected with the silver disc.

R, S, two tongues of zinc connected with the zinc disc.

Z, upper plate of the condensor, made of zinc.

V, lower plate, made of silver, resting on the silver leaf electrometer, which in the upper part, where in contact with the condensor, is mounted in silver.

In another part of the plate are represented two cups containing a prepared frog, *b* being the body, and *d* the legs. This is made use of in the experiments described in § 12.

charge on being directed on to the frog did not produce any visible contraction.

11. I next immersed simultaneously the tongues of the two dissimilar metals in the same cup of acidulated water, in order to see if the chemical action, exercised with different energy on the two metals, would charge the jar, as contact does; but I did not obtain indications of any tension.

The experiment was tried of immersing the zinc tongue of the zinc coating into concentrated sulphuric acid, as well as moderately and very much diluted, dipping at the same time the tongue of the silver coating into distilled water in contact with the acid. But without effect. Does this arise from the jar charging itself and discharging itself at the same time? No, for on immersing the silver wire in one glass of acidulated water, and the zinc wire in another, the effect, as before, was null. And, besides, I would ask those who should make this objection how it is that it does not happen that the jar charges and discharges itself as well when the two coatings are brought into metallic communication with each other. Whereas every time the silver wire was brought into contact with the external coating, either directly or by means of another metal, or a piece of charcoal, or any other solid electrometer whatever, the jar instantly became charged, giving indications of its tension with the electrometer as has been described.

The case in which the jar charges and discharges itself occurs when we bring the tongues of the coatings into contact with each other at the points of their immersion. For in the act of separating them, they being in communication through a liquid conductor, the electric equilibrium, which had been disturbed by the mutual contact of two dissimilar metals, is instantly restored.

12. I prepared a frog in such a manner that the lower limbs were attached to a portion of the trunk only by means of the sciatic and crural nerves. Then, having brought the silver wire proceeding from the internal coating into connection with the trunk, and the strip of zinc proceeding from the external coating into connection with the legs, I did not observe any contraction, neither did I obtain any on touching the nerves and the thighs, or the nerves and the feet, with the above wires. But I had no sooner brought the two coatings into metallic contact with each other, than, on connecting the trunk with the external coating and the legs with the internal, the frog was convulsed. And here, too, it was precisely as it obtains with the ordinary jar when charged to an extremely low tension, namely, that when the current was transmitted from the limbs to the trunk, instead of in the opposite direction, either no contraction took place, or it was extremely feeble, and only noticed when the frog was recently prepared. (§ 6.)

The above experiments were varied and repeated with the same results at least forty times in the space of three-quarters of an hour, the same frog being employed throughout. I repeated the experiment four times upon the same frog, even after an interval of three hours, and the contractions took place as before, only they

were very much feebler. This was on the 31st of January of this year. Similar experiments were instituted with the same results in March and April following.

13. These experiments, which indeed demonstrate that the contact of solid heterogeneous conductors develops electricity, may be, it is true, performed with a less expensive apparatus, although the arrangement that we have described may be perhaps better adapted for varying and studying the experiments themselves. The two discs, for instance, may be omitted, and the silver wire and zinc strip may be managed directly with the hands if protected by insulating gloves. In lieu of the condensor of zinc and silver we may employ one of zinc and brass, were it not that the tension indicated by the latter is something less, owing to the brass, on coming into contact with the silver, becoming electrified in an opposite state to the silver of the coating. We may further make use of the ordinary condensor, whose plates are both of brass, provided a moist card is inserted between the zinc thread and the brass plate. I think we can also dispense with a coating of zinc, and may employ tin-foil instead, which is far easier to manage and not so expensive, for I have observed that the jar in question, when charged with a single pair of silver and tin plates, affected a prepared frog. It must be borne in mind, however, that if we are desirous of obtaining equal effects, it would be necessary that the jar used for tin and silver coatings should present a much greater extent of surface than when coatings of silver and zinc are used.

14. Having remarked, even after charging the condensor several times with the Leyden jar, without the charge of the latter being, however, renewed, that the frog was affected (though certainly to a very trifling amount) on receiving the residual charge, I was desirous of ascertaining if one would obtain a similar effect by means of a condensor with plates of dissimilar metals. I therefore endeavoured to charge the above condensor by bringing its two plates into mutual metallic contact, and then making the zinc plate communicate with the trunk, and the silver one with the limbs of the frog; but to no purpose. It was necessary to charge this instrument with a two-fold tension in order to affect the frog by it. Having, however, charged it from two pairs of brass and zinc plates, I succeeded in obtaining contractions, although somewhat less marked than those that the jar in question produced. And these contractions were likewise obtained with the condensor of brass and zinc when charged by the above-mentioned two pairs of plates.

If, instead of charging the condensor by putting the two discs in mutual metallic communication, I charged it by touching the silver disc with a strip of zinc, and by connecting the latter by means of a moist conductor with the disc of zinc, there were not in this case either, contractions produced on directing the discharge on to the frog. This, however, shows that even when a fluid is present the charge of the condensor is not sensibly increased.

15. I took two condensers of brass and zinc, and having placed

the two zinc plates in metallic communication with each other, as also the two brass plates in a similar manner, I established a metallic communication between one of the brass plates and one of those of zinc, upon which both the condensers received the ordinary charge of about nine degrees, but even with this charge, although the quantity of electricity thrown into motion could not but be nearly twice as great as when a single condensor is charged, the frog was not affected in the slightest degree. But on charging these two condensers when thus connected with two pairs of brass and zinc, in other words with a two-fold tension, I obtained contractions which were even stronger than when I used but one condensor charged to the same intensity.

From these experiments we see that an increase also in the amount of electricity tends to strengthen the shock, but not to the same extent as tension.

16. I have also succeeded in charging this jar little by little, that is to say by repeated contacts. I placed a plate of zinc and a plate of silver in an insulated cup of water, taking care that they did not touch and that they were not wholly immersed. I then touched the zinc plate with the tongue of the silver coating, and next the silver plate with the tongue of the zinc coating, and having repeated this alternately eight or ten times the jar I found was charged.

If these plates of zinc and silver, instead of dipping into the same cup, dipped into two different ones, but connected together by means of the frog, or other moist conductor, or even by a simple metallic arc, the experiment succeeded as before.

The jar may likewise be charged little by little by touching the zinc plate with the silver coating, as in the previous experiment, and then touching with the zinc coating the water in communication with the zinc plate.

Then touching simultaneously the zinc plate in question with the silver coating, and either the silver plate or the water into which it dips with the zinc coating, the jar became instantly charged. In which case if a frog recently prepared is used as the moist conductor between the cups, it contracts in the act of the jar becoming charged; and then it also contracts in the ordinary manner when, on connecting the two coatings with the water in the two cups, the charge of the jar is sent through it.

17. This last experiment may also be easily repeated with the condensor. Having prepared a somewhat large and active frog let the trunk be laid on a plate of zinc, and one leg on a plate of silver. Let one of these two plates be held between two fingers furnished with insulating gloves, and the other between two other fingers, and the contractions of the frog will be observed both at the moment the condensor receives the charge of the two pairs on making the zinc disc touch the plate of silver, and the silver disc the plate of zinc, and also at the moment the condensor is discharged by touching the zinc disc with the zinc plate, and the silver disc with the plate of the same metal.

The experiment succeeds also very well with a condenser and plates of brass and zinc, only the contractions under similar circumstances are more feeble.

18. A tongue of zinc being adapted to the silver coating, and one of silver to that of zinc, the experiments described in § 16 were repeated, and similar effects produced.

I have, moreover, succeeded more than once in charging the jar little by little by touching an insulated plate of zinc a hundred, two hundred, or three hundred times with each of the two coatings alternately. And the proof of this was seen in the contractions, feeble it is true, but yet quite perceptible, produced in the prepared frog.

19. There is another way also in which this jar may be charged. Putting the silver coating of the jar in connection with the zinc plate of the condenser, and the zinc coating with the silver plate, the condenser is charged with double the tension it has when electrified with the charge of the jar, and this arises from their being here as it were two pairs of plates, and the two coatings present a considerable surface. Leaving the followers of De la Rive to consider by what chemical action they will re-produce the effects of these two pairs; we would wish to observe that in charging the condenser in this manner, we at the same time charge the jar in a slight degree. This is indicated, although with a very minute divergence, by the electrometer supporting the condenser itself, when, after discharging it, the silver coating is brought into connection with the silver plate, and the zinc coating with that of zinc.

For if the operation of charging the condenser with the two coatings of the jar is repeated several times, and if we then discharge it without discharging the jar, it will be seen that the above condenser becomes less charged every time, a proof that the charge of the jar goes on increasing and indeed after nine or ten times the jar becomes charged to that extent that it causes the leaves of the electrometer to diverge five or six degrees.

The experiment is performed more quickly, if the condenser is discharged consecutively by forming an arc between the two plates with the finger and thumb of one hand, every time that it receives the charge from the two coatings of the jar.

20. A condenser then whose plates are of silver and zinc, and a Leyden jar whose two coatings are formed of these same two metals become mutually charged when the zinc coating touches the silver plate of the condenser, the silver coating touching the zinc plate at the same time; and if the state of tension in which the jar is, is much lower than that of the condenser, it is owing to the extent of surface being so much less in the latter. But if the two surfaces inserted between the coatings are equal in size, the tension of the charges they receive will be equal.

The following experiment was instituted several times, with two condensers of brass and zinc that presented as near as may be an equal extent of surface, inasmuch as the plates were of the same size, and the intervening stratum as nearly as possible of the same thick-

ness. I brought the brass disc of the first condenser into communication with the zinc disc of the second, by means of a strip of zinc, or any other metal, and at the same time I connected the zinc disc of the first with the brass disc of the second, by means of a strip of brass or other metal; and on examining the tension with the electrometers that supported them, I have always found, within a very little, an equal divergence in the leaves.

In like manner a pair of Franklinian tables with heterogeneous coatings of silver and zinc, and both having in each of their coatings a surface of ten square decimeters, and glass borders of equal width, became reciprocally charged with a similar tension.

I also adapted to the jar in question an arrangement which enabled me to charge and discharge it many times in succession with some rapidity, that is to say about a hundred times in a second. On exposing a frog prepared in the usual manner to these repeated electric discharges, and which formed as it were an uninterrupted current, it was much more strongly convulsed than when assailed with only one of these discharges, and it remained contracted for some time, pretty much as when submitted to a continuous current from a weak simple voltaic circuit.

Neither the reo-electrometer nor the galvanometer were in the least moved on being subjected to the above discontinuous current; and yet there were indications of chemical action. I think the current circulating in my apparatus is too feeble to produce that kind of phenomena.

At any rate the constructing a battery of jars, or what would be better still of Franklinian tables with heterogeneous coatings of fifty or sixty times greater surface than my jar, and then adapting an arrangement to it by which it might be charged and discharged alternately several hundred times in a second, and then with this apparatus imitating the whole of the effects, voltaic circuit would have, I think, but one difficulty in its way, namely, meeting with some philosopher whom we could convince of its utility, and who was possessed of the means of executing it. It is enough for me to have discovered by the form I have given to my apparatus, the means of demonstrating by simple and conclusive experiments, that metals of opposite kinds become electrified when brought into contact.*

* A notice of the arrangement forming the subject of this digression was contained in a letter of the 10th May, 1838, addressed to Dr. P. Marianini, and which subsequently appeared in the *Gazetta Piemontese* of that year, No. 232.

It is now more than forty years since Volta showed that the simple contact of dissimilar metals produced electrical phenomena. The pile which bears that eminent philosopher's name was invented from the discovery of that fact. We have discovered, more than ten years ago, that two pieces of the same kind of metals produces the same effect.—EDITOR.

XXXIII.—*On Electrochemical Piles and their use in making metallic sulphurets by cementation, and other products.* By M. BECQUEREL.*

I informed the academy some years back of several electro-chemical processes by means of which we obtained crystallized sulphurs of copper, silver, iron, lead, &c.; my attention has been recalled to this subject by examining several pieces of silver which have been entirely changed into sulphuret, in consequence of being left for some time down a privy. The surface of these pieces was covered with small octahedrated crystals, and their texture was crystalline. The transformation of the metallic silver into sulphuret must of necessity have been effected by cementation, since the pieces still retain their form.

Wishing to imitate this transformation by means of electro-chemical actions, I was obliged to change the process I have hitherto made use of, as it would not answer for the solution of the question; that which I have substituted for it, has led me to several results, important to chemistry in general, and electro-chemistry in particular.

The apparatus I used consisted of a certain number of pipes, bent into the form of U. twelve or fifteen centimeters high, and one in diameter. Each tube was arranged in the following manner: clay moistened with water was put as usual in the bottom of the tube so as to occupy a space of about six or seven centimeters, a cotton plug was then placed upon the clay in each branch, to prevent the products formed, from mixing with the clay. In one of the branches was poured a solution of proto-sulphuret of potassium, in the other, a tolerably well concentrated solution of nitrate of copper. In the first, a plate of silver, and in the other a plate of copper was immersed, six tubes were arranged in this manner, I then took a board 15 millimetres in thickness, and of a convenient length and breadth, and notched it so as to fasten it with cement to the bent part of each tube. These tubes were so arranged, that the branch containing the nitrate of copper was opposite that which contained the proto sulphuret of potassium. These arrangements being accomplished, a certain number of elements were collected together so as to make a pile. It is sufficient for this to connect the copper of the first with the silver of the second, and the copper of the second with the silver of the third, and so on to the last, then to close the circuit, I put the copper of the last in connection with the silver of the first. This is the pile to which I gave the name of the electro-chemical pile, because it performs the office of a pile, acting entirely on the chemical reactions in the interior of the tubes. We may conceive that very energetic apparatus may be thus formed, whose effects become sensible in a very short time, especially when the clay which is on the side of the sulphuret, is moistened with a solution of this sulphuret, and that in the other branch with a solution of nitrate. By means of this arrangement, the two solutions act on each other immediately.

* Translated by J. H. Lang, Esq., (from the *Comptes Rendus*, No. 20, 1839.)

Five or six hours after one of my apparatus began to act, crystals of metallic copper were observed on the copper plates, a characteristic sign of the existence of electro-chemical action. Twelve hours afterwards, the silver plates were covered with crystals, which being analyzed were found to be composed of silver and sulphur. The action was continued without interruption for upwards of a fortnight, when the plates without having lost their forms were changed into sulphuret, whose appearance was the same as that of pieces of silver which have remained for a certain number of years down a privy. We obtained the same results with one element, but it took longer; nothing is more simple than to explain the effects produced: the silver in each tube being attached by the sulphur, takes the negative electricity which it transmits to the copper: on the other hand, the sulphuret of potassium in its reaction on the nitrate, takes the negative electricity, which it transmits to the silver, and by that means to the copper. Hence the latter is doubly negative as the silver itself is doubly positive; a similar effect taking place in each tube, it follows that when they are united in a pile, the action must be energetic.

We will now consider the effects produced. The nitrate of copper is decomposed by the plate of the same metal, which is negative: the oxygen and nitric acid are carried over to the silver in the proto sulphuret of potassium. The oxygen oxidises, the potassium and nitric acid combine with the potassa formed, while the sulphur is carried over to the silver and combines with it, forming sulphuret which crystallizes by reason of the slow actions. When once the surface of the silver is covered with a bed of sulphuret, which only sticks to it, the sulphur slips between the interstices of the small crystals which are formed, and gives rise to a second bed of crystals of sulphuret, and so on to the centre of the plate, which increases in volume without altering its form; in consequence of these successive deposits the spaces between which are invisible even with a microscope. The junction of all these deposits forms a compact mass, having a crystallized texture. This is a real cementation, and it is probable that those which occur in nature are produced by a similar mode of action. It is rightly conceived that an electric current traversing bodies may deposit some elements in their interior, when the molecular interstices are sufficiently open for these elements to pass. In this I state a fact and not a theoretical idea.

Before passing to the results obtained with other metals, I must return to the changes which the pieces of silver in the privy undergo.

We know that silver experiences a rapid alteration in a medium where there are sulphurets which can give it a portion of their sulphur, whilst the other parts are oxidised. If the action be slow, the mass of silver is changed into a sulphuret having a crystallized structure, without the form being changed, although the bulk is increased.

In privies this transformation is frequently effected by sulphurets which are found there. In order for the electro-chemical action to operate as in the preceding experiment, it only requires the silver

to be in contact with a tolerable conductive carbonaceous matter, and that it has air to replace the oxygen which proceeds from the reduction of the oxide of copper in the before mentioned experiment. Hence in my experiments, I merely unite the most favorable circumstances for the production of the phenomenon, which are not always found united in nature.

We will now pass on to the formation of sulphurets of copper and lead; still employing the action of the electro-chemical piles, whose force may be increased at pleasure according to the energy of the affinities we wish to produce.

With copper the effects vary, according as we operate with a solution of concentrated persulphuret of potassium, or an equally concentrated protosulphuret. In the first case, after a few days we began to observe sometimes, on the sides of the tube, beautiful white radiated spires of a double insoluble sulphuret of potassium, quite incapable of being changed by the air. This compound, supplied with nitric acid, gives nitrate of potassa and nitrate of copper with a liberation of nitrous gas. The plate was sometimes covered with crystals of sulphur and small tubercules of the same substance: nitrate of potassa was also found in the solution. These effects were obtained especially when in the latter a small quantity of sugar was added, so as to produce a reaction which I shall explain in another memoir. If we continue the operation, nitric acid and oxygen being constantly added, act on the products formed, decomposing them and giving rise to sulphate and nitrate of copper, then to variegated crystals of sulphuret of copper mixed with sulphur in spires. Hence the experiment must be stopped in time, if we wish to preserve the first formed products. With protosulphuret of potassium, the effects are the same as with silver, viz., that a crystallised sulphuret of copper of a grey metallic appearance is formed, in microscopic crystals, from the minuteness of which it is difficult to determine the form.

This effect was equally obtained with persulphuret when the electric current was of a certain intensity. I remark, that when we use an electro-chemical apparatus composed of from three to six elements, it is often difficult to foresee the effects which will be produced since they depend on unforeseen circumstances relative to the conductivity of the different elements.

Lead, with proto sulphuret of potassium, has at first reactions analogous to those of silver but with this difference, that the sulphuret is at first pulverulent; but when the solution becomes less concentrated, it forms tuberculous masses of brilliant sulphuret of lead, of a crystallized appearance, similar to that of black lead; a double sulphuret of lead and potassium is sometimes obtained in white needles.

The substances formed have generally the appearance of those corresponding to them in nature.

From the facts stated in this memoir we may conclude, that simple electro-chemical machines may be connected in piles, of which the decomposing action in each machine, depends on the

number of these elements, and are capable of producing many compounds analogous to mineral substances. These piles which act with much more energy than the simple machines I have hitherto used, act like the latter for a tolerable time, and with an energy of action with which electro-chemistry will henceforth be able to take part in calling the attention to preserve successively the compounds produced, if we do not wish to see them vanish to be replaced by others.

XXXIV.—*Note on the incapability of water to conduct voltaic currents without being decomposed.* BY W. R. GROVE.*

It is of the utmost importance to the electro-chemical theory, to decide whether the electrolytes can conduct electric currents without being decomposed. The well known experiment of Dr. Faraday is not conclusive on this point, for if the explanation given by M. Becquerel, of the phenomenon of polarised electrodes be admitted, it proves that feeble currents are capable of decomposing water.

Against this explanation the following is worthy of great attention. Platina electrodes being immersed in water not acidulated with nitric acid, no ebullition of gas could be procured, with a single voltaic pair, however long the experiment lasted. However when copper electrodes were substituted for these, the decomposition was effected, although copper alone is incapable of decomposing acidulated water, and the two electrodes being of the same metal could add nothing to the initial current, since the current produced by them being in opposite directions destroy one another: the only difference in the latter case is that, there exists no longer any necessity for disengaging each element to a gaseous state. Hence it appears that the feeble current of a single pair is sufficient to separate the elements of water, but not to continue this action by disengaging them under the gaseous form† in order to make room for new portions. If it be thus, the decomposition must soon stop, and if the conduction depends on the decomposition, it can no longer take place. This question is easily solved by experiment.

Dr. Faraday remarked, that when the platina plates in his experiment (*Exp. Res.* 1026) remained in contact for some time, and when put in contact with a solution of iodide of potassium, the iodide was

* Translated by J. H. Lang, Esq., (from the *Comptes Rendus*, No. 20, 1839.)

† The reason why more intensity is required to disengage singly the elements in a gaseous state, than to separate them, is a very complicated one: at present I only mention the fact, which may be established by a number of experiments.

not immediately decomposed. He has rightly attributed this to the polarised state of the exciting plates, but he has not remedied this inconvenience, since up to this time he was unacquainted with the method of producing a constant current.

I repeated Dr. Faraday's experiment with some alterations as follows: the zinc and copper pair of metals I used were for a constant current, separated by a porous diaphragm, and charged with diluted sulphuric acid and sulphate of copper, I guarded against the evaporation, in the place of operation, by hermetically sealing the ends of the tube containing the platina plates.

Iodide of potassa was then subjected to a constant current produced by these metals, and traversing the water of the tube, the iodide was decomposed but feebly; it was removed and the contact, established for some minutes between the platina points.

The iodide being replaced was not decomposed for about the space of a minute; after which a very feeble decomposition commenced and continued. Making this last experiment with a short wired galvanometer instead of the iodide, I observed no deviation although the instrument was extremely delicate. But with a Gourjon's long wired galvanometer, I obtained a deviation of nearly 8° . when the mercury cups of the galvanometer were connected together for some minutes by means of an amalgamated copper wire, and the latter being suddenly taken away, the current traversed the wire of the instrument. Instead of a sudden deviation as usual, the needle was not effected for two seconds, the deviation then commenced very slowly and continued till it arrived at 8° , where it finally stopped. When the tube containing the electrodes was suddenly inverted, the decomposition and deviation were excessive, and even the polarised electrodes gave an instantaneous deviation of 85° .

These experiments satisfactorily confirm the above mentioned ideas. Thus the contact having remained long enough for the electrodes to be covered to their greatest extent with the transposed elements, on introducing iodide there was no current: the effect of this stoppage is to allow these elements a reaction; the electrodes by this means are depolarised, and at a certain point, the initial current regains its power and passes on decomposing a fresh the water and iodide and repolarising the electrodes: so that a kind of intermitting action is established, which however has all the appearance of a continual decomposition. With the short wired galvanometer there is little interruption, and consequently no apparent deviation; but with a very delicate galvanometer some effect is perceived, since the long wire give a slight resistance to the current compared to that of iodide.

In this last experiment, first the perfect immoveability of the needle, and the feeble deviation which took place afterwards (when we consider the sensibility of the instrument), appears to me con-

clusive, and to prove definitively that in water there is no conductivity without decomposition.*

The following are some experiments which tend to strengthen these considerations.

We know from M. Becquerel that if a platina plate be placed in nitric acid and another in a solution of caustic potassa, which is connected with the acid by means of a porous diaphragm, as soon as the communication is effected we have a tolerably evident electric current, the alkali taking negative and the acid positive electricity; while with the same arrangement, substituting sulphuric or hydro-chloric for the nitric acid, a very feeble current is produced.

The preceding considerations induced me to believe that in these last experiments the electricity disengaged by the reaction of the acid on the alkali could not be conducted without the liberation of two elements in a gaseous state; consequently, if plates of an oxidisable† metal were used instead of platina, a more energetic current would be produced. Having substituted them the results were very striking, not only was there a current, but such an energetic one that it almost equalled that which results from the ordinary arrangement of zinc and platina.

I tried the three following metals, iron, copper, and zinc; two plates of the *same metal* having been immersed respectively in sulphuric acid and a solution of potassa, or in hydro-chloric acid and a solution of potassa, a very decided and constant electric current ensued, but much more intense with the zinc than the two other metals, the energy increased when the sulphuric acid was diluted with half water.

But the most remarkable fact is, that the plate of zinc which was in the acid, although much more attacked chemically than that which was in the alkali, always took positive electricity, i. e., it represented the copper in an ordinary voltaic combination.

Like phenomena were exhibited with nitric acid: in this acid, inactive and active iron, zinc, and copper, always take positive electricity with respect to the same metals in potassa, or in sulphuric and hydro-chloric acids.

I am about to try several experiments in this class of phenomena, having in view the improvement of voltaic piles. Hitherto I have found no better combination than that I had the honor of presenting to the Academy, 15th April, except that in it a solution of marine salt may be advantageously substituted for the acidulated water.

* The observations and experiments above-mentioned apply only to water; they might, however, be extended in some degree to all electrolytes in which water is the solvent.

† Perhaps I ought to mention that Sir H. Davy made this experiment, but he attributed the current to the chemical action of the alkali on the metal, which he considered as stronger than that of the metal.

XXXV.—Experimental Researches in Electricity.—Thirteenth Series. By MICHAEL FARADAY, Esq., D.C.L. F.R.S. Fullerman Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acad. of Sciences, Paris, Petersburg, Florence, Copenhagen, Berlin, &c. &c.

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§. 18. *On Induction (continued).* ¶ ix. *Disruptive discharge (continued).*—Peculiarities of positive and negative discharge either as spark or brush—Glow discharge—Dark discharge.—¶ x. *Convection, or carrying discharge.* ¶ xi. *Relation of a vacuum to electrical phenomena.* §. 19. *Nature of the electrical current.*

¶ ix. *Disruptive discharge (continued).*

1480. Let us now direct our attention to the general difference of the positive and negative disruptive discharge, with the object of tracing, as far as possible, the cause of that difference, and whether it depends on the charged conductors principally, or on the interposed dielectric; and as it appears to be great in air and nitrogen (1476.), let us observe the phenomena in air first.

1481. The general case is best understood by a reference to surfaces of considerable size rather than to points, which involve (as a secondary effect) the formation of currents (1562.). My investigation, therefore, was carried on with balls and terminations of different diameters, and the following are some of the principal results.

1482. If two balls of very different dimensions, as for instance one, half an inch, and the other three inches, in diameter, be arranged at the ends of rods so that either can be electrified by a machine and made to discharge by sparks to the other, which is at the same time uninsulated; then, as is well known, far longer sparks are obtained when the small ball is positive and the large ball negative, than when the small ball is negative and the large ball positive. In the former case, the sparks are 10 or 12 inches in length; in the latter an inch or an inch and a half only.

1483. But previous to the description of further experiments, I will mention two words, for which with many others I am indebted to a friend, and which I think it would be expedient to introduce and use. It is important in ordinary inductive action, to distinguish at which charged surface the induction originates and is sustained: i. e. if two or more metallic balls, or other masses of matter, are in inductive relation, to express which are charged originally, and which are brought by them into the opposite electrical condition. I propose to call those bodies which are originally charged, *inductric* bodies; and those which assume the opposite state, in consequence of the induction, *inducteous* bodies. This distinction is not needless because there is any difference between the sums of the *inductric* and the *inducteous* forces; but principally because, when a ball A

is inductive, it not merely brings a ball B, which is opposite to it, into an inductive state, but also many other surrounding conductors, though some of them may be a considerable distance off, and the consequence is, that the balls do not bear the same precise relation to each other when, first the one, and then the other, is made the inductive ball; though, in each case, the *same ball* be made to assume the *same state*.

1484. Another liberty which I may also occasionally take in language I will explain and limit. It is that of calling a particular spark or brush, *positive* or *negative*, according as it may be considered as *originating* at a positive or a negative surface. We speak of the brush as positive or negative when it shoots out from surfaces previously in those states; and the experiments of Mr. WHEATSTONE go to prove that it *really begins* at the charged surface, and from thence extends into the air (1437. 1438.) or other dielectric. According to my view, *sparks* also originate or are determined at one particular spot (1370.), namely, that where the tension first rises up to the maximum degree; and when this can be determined, as in the simultaneous use of large and small balls, in which case the discharge begins or is determined by the latter, I would call that discharge which passes *at once*, a positive spark, if it was at the positive surface that the maximum intensity was first obtained, or a negative spark, if that necessary intensity was first obtained at the negative surface.

1485. An apparatus was arranged, as in fig. 15. (Plate III.): A and B, were brass balls of very different diameters attached to metal rods, moving through sockets on insulating pillars, so that the distance between the balls could be varied at pleasure. The large ball A, 2 inches in diameter, was connected with an insulated brass conductor, which could be rendered positive or negative directly from a cylinder machine: the small ball B, 0.25 of an inch in diameter, was connected with a discharging train (292.) and perfectly uninsulated. The brass rods sustaining the balls were 0.2 of an inch in thickness.

1486. When the large ball was *positive* and inductive (1483.), negative sparks occurred until the interval was 0.49 of an inch; then mixed brush and spark between that and 0.51; and from 0.52 and upwards, negative brush alone. When the large ball was made *negative* and inductive, then positive spark alone occurred until the interval was as great as 1.15 inches; spark and brush from that up to 1.55; and to have the positive brush alone, it required an interval of at least 1.65 inches.

1487. The balls A and B were now changed for each other. Then making the small ball B inductive *positively*, the positive sparks alone continued only up to 0.67; spark and brush occurred from 0.68 up to 0.72; and positive brush alone from 0.74 and upwards. Rendering the small ball B inductive and *negative*, negative sparks alone occurred up to 0.40; then spark and brush at 0.42; whilst from 0.44 and upwards the noisy negative brush alone took place.

1488. We thus find a great difference as the balls are rendered in-

ductric or inductive; the small ball rendered *positive* inductively giving a spark nearly twice as long as that produced when it was charged positive inductrically, and a similar difference, though not, under the circumstances, to the same extent, was manifest when it was rendered *negative*.

1489. Another result is, that the small ball rendered positive gives a much longer spark than when it is rendered negative, and that the small ball rendered negative gives a brush more readily than when positive, in relation to the effect of increasing distance.

1490. When the interval was below 0.4 of an inch, so that the small ball should give sparks, whether positive or negative, I could not observe that there was any constant difference, either in their ready occurrence or the number which passed in a given time. But when the interval was such that the small ball when negative gave a brush, then the discharges from it, as separate negative brushes, were far more numerous than the corresponding discharges from it when rendered positive, whether those positive discharges were as sparks or brushes.

1491. It is, therefore, evident that, when a ball is discharging electricity in the form of brushes, the brushes are far more numerous, and each contains or carries off far less electric force when the electricity so discharged is negative, than when it is positive.

1492. In all such experiments as those described, the point of change from spark to brush is very much governed by the working state of the electrical machine and the size of the conductor connected with the discharging ball. If the machine be in strong action and the conductor large, so that much power is accumulated quickly for each discharge, then the interval is greater at which the sparks are replaced by brushes; but the general effect is the same.

1493. These results, though indicative of very striking and peculiar relations of the electric force or forces, do not show the relative degrees of charge which the small ball acquires before discharge occurs, i. e. they do not tell whether it acquires a higher condition in the negative or in the positive state, immediately preceding that discharge. To illustrate this important point I arranged two places of discharge as represented fig. 16. A and D are brass balls two inches in diameter, B and C are smaller brass balls 0.25 of an inch in diameter; the forks L and R supporting them were of brass wire 0.2 of an inch in diameter: the space between the large and small ball on the same fork was 5 inches, that the two places of discharge *n* and *o* might be sufficiently removed from each other's influence. The fork L was connected with a projecting cylindrical conductor, which could be rendered positive or negative at pleasure, by an electrical machine, and the fork R was attached to another conductor, but thrown into an uninsulated state by connection with a discharging train. The two intervals or places of discharge *n* and *o* could be varied at pleasure, their extent being measured by the occasional introduction of a diagonal scale. It is evident that as the balls A and B connected with the same conductor are always charged at once, and that discharge may take place to either of the

balls connected with the discharging train, the intervals of discharge n and o may be properly compared to each other, as respects the influence of large and small balls when charged positively and negatively in air.

1494. When the intervals n and o were each made = 0.9 of an inch, and the balls A and B inductive *positively*, the discharge was all at n from the small ball of the conductor to the large ball of the discharging train, and mostly by positive brush, though once by a spark. When the balls A and B were made inductive *negatively*, the discharge was still from the same small ball, at n , by a constant negative brush.

1495. I diminished the intervals n and o to 0.6 of an inch. When A and B were inductive *positively*, all the discharge was at n as a positive brush: when A and B were inductive *negatively*, still all the discharge was at n , as a negative brush.

1496. The facility of discharge at the positive and negative small balls, therefore, did not appear to be very different. If a difference had existed, there were always two small balls, one in each state, that the discharge might happen at that most favourable to the effect. The only difference was, that one was in the inductive, and the other in the inductive state, but whichever happened for the time to be in that state, whether positive or negative, had the advantage.

1497. To counteract this interfering influence, I made the interval n = 0.79 and interval o = 0.58 of an inch. Then, when the balls A and B were inductive *positive*, the discharge was about equal at the two intervals. When, on the other hand, the balls A and B were inductive *negative*, there was discharge, still at both, but most at n , as if the small ball *negative* could discharge a little easier than the small ball *positive*.

1498. The small balls and terminations used in these and similar experiments may very correctly be compared, in their action, to the small balls and ends when electrified in free air at a much greater distance from conductors, than they were in those cases from each other. In the first place, the discharge, even when as a spark, is, according to my view, determined, and, so to speak, begins at a spot on the surface of the small ball (1874.), occurring when the intensity there has risen up to a certain maximum limiting degree (1370.); this determination of discharge at a particular spot first, being easily traced from the spark into the brush, by increasing the distance, so as, at last, even to render evident the time which is necessary (1436. 1428.). In the next place, the large balls which I have used might be replaced by larger balls at a still greater distance, and so, by successive degrees, may be considered as passing into the sides of the rooms; these being under general circumstances the inductive bodies, whilst the small ball rendered either *positive* or *negative* is the inductive body.

1499. But, as has long been recognised, the small ball is only a blunt end, and, electrically speaking, a point only a small ball; so that when a point or blunt end is throwing out its brushes into the

air, it is acting exactly as the small balls have acted in the experiments already described, and by virtue of the same properties and relations.

1500. It may very properly be said with respect to the experiments, that the large negative ball is as essential to the discharge as the small positive ball, and also that the large negative ball shows as much superiority over the large positive ball (which is inefficient in causing a spark from its opposed small negative ball) as the small positive ball does over the small negative ball; and probably when we understand the real cause of the difference, and refer it rather to the condition of the particles of the dielectric than to the sizes of the conducting balls, we may find much importance in such an observation. But for the present, and whilst engaged in investigating the point, we may admit, what is the fact, that the forces are of higher intensity at the surfaces of the smaller balls than at those of the larger (1372. 1374.); that the former, therefore, determine the discharge, by first rising up to that exalted condition which is necessary for it; and that, whether brought to this condition by induction towards the walls of a room or the large ball I have used, these may fairly be compared one with the other in their influence and actions.

1501. The conclusions I arrive at are: first, that when two equal small conducting surfaces equally placed in air are electrified, one positively and the other negatively, that which is negative can discharge to the air at a tension a little lower than that required for the positive ball: second, that when discharge does take place, much more passes at each time from the positive than from the negative surface (1491.). The last conclusion is very abundantly proved by the optical analysis of the positive and negative brushes already described (1468.), the latter set of discharges being found to recur five or six times oftener than the former.*

1502. If, now, a small ball be made to give brushes or brushy sparks by a powerful machine, we can, in some measure, understand and relate the difference perceived when it is rendered positive or negative. It is known to give when positive a much larger and more powerful spark than when negative, and with greater facility (1482.); in fact, the spark, although it takes away so much more electricity at once, commences at a tension higher only in a small degree, if at all. On the other hand, if rendered negative, though discharge may commence at a lower degree, it continues but for a very short period, very little electricity passing away each time. These circumstances are directly related, for the extent to which the positive spark can reach, and the size and extent of the positive brush, are consequences of the capability which exists of much electricity passing off at one discharge from the positive surface (1468. 1501.).

1503. But to relate these effects only to the form and size of the

* A very excellent mode of examining the relation of small positive and negative surfaces would be by the use of drops of gum water, solutions, or other liquids. See onwards (1581. 1593.).

conductor, would, according to my notion of induction, be a very imperfect mode of viewing the whole question (1523.). I expect that the effects are due altogether to the mode in which the particles of the interposed dielectric polarize, and I have already given some experimental indications of the differences presented by different dielectrics in this respect (1475. 1476.). The modes of polarization, as I shall have occasion hereafter to show, may be very diverse in different dielectrics. With respect to common air, what seems to be the consequence of a superiority in the positive force at the surface of the small ball, may be due to the more exalted condition of the negative polarity of the particles of air, or of the nitrogen in it (the negative part being, perhaps, more compressed, whilst the positive part is more diffuse, or *vice versâ*); for such a condition could determine certain effects at the positive ball which would not take place to the same degree at the negative ball, just as well as if the positive ball had possessed some special and independent power of its own.

1504. That the effects are more likely to be dependent upon the dielectric than the ball, is supported by the character of the two discharges. If a small positive ball be throwing off brushes with ramifications ten inches long, how can the ball effect that part of a ramification which is five inches from it? Yet the portion beyond that place has the same character as that preceding it, and no doubt has that character impressed by the same general principle and law. Looking upon the action of the contiguous particles of a dielectric as fully proved, I see, in such a ramification, a propagation of discharge from particle to particle, each doing for the one next it what was done for it by the preceding particle, and what was done for the first particle by the charged metal against which it was situated.

1505. With respect to the general condition and relations of the positive and negative brushes in dense or rare air, or in other media and gases, if they are produced at different times and places, they are of course independent of each other. But when they are produced from opposed ends or balls at the same time, in the same vessel of gas (1470. 1477.), they are frequently related; and circumstances may be so arranged that they shall be isochronous, occurring in equal numbers in equal times; or shall occur in multiples, i. e. with two or three negatives to one positive; or shall alternate, or be quite irregular. All these variations I have witnessed; and when it is considered that the air in the vessel, and also the glass of the vessel, can take a momentary charge, it is easy to comprehend their general nature and cause.

1506. Similar experiments to those in air (1485. 1493.) were made in different gases, the results of which I will describe as briefly as possible. The apparatus is represented fig. 17. consisting of a bell-glass eleven inches in diameter at the widest part, and ten and a half inches high up to the bottom of the neck. The balls are lettered, as in fig. 16, and are in the same relation to each other; but A and B were on separate sliding wires, which, however, were

generally joined by a cross wire, n , above, and that connected with the brass conductor, which received its positive or negative charge from the machine. The rods of A and B were graduated at the part moving through the stuffing-box, so that the application of a diagonal scale applied there, told what was the distance between these balls and those beneath them. As to the position of the balls in the jar, and their relation to each other, C and D were three and a quarter inches apart, their height above the pump plate five inches, and the distance between any of the balls and the glass of the jar one and three quarter inches at least, and generally more. The balls A and D were two inches in diameter, as before (1493): the balls B and C only 0.15 of an inch in diameter.

Another apparatus was occasionally used in connection with the one just described, being an open discharger (fig. 18.), by which a comparison of the discharge in air and that in gases could be obtained. The balls E and F, each 0.6 of an inch in diameter, were connected with sliding rods and other balls, and were insulated. When used for comparison, the brass conductor was associated at the same time with the balls A and B of figure 17 and ball E of this apparatus (fig. 18); whilst the balls C D and F were connected with the discharging train.

1507. I will first tabulate the results as to the *restraining power* of the gases over discharge. The balls A and C (fig. 17.) were thrown out of action by distance, and the effects at B and D, or the interval n in the gas, compared with those at the interval p in the air, between E and F (fig. 18.). The table sufficiently explains itself. It will be understood, that all discharge was in the air, when the interval there was less than that expressed in the first or third columns of figures; and all the discharge in the gas, when the interval in air was greater than that in the second or fourth column of figures. At intermediate distances the discharge was occasionally at both places, i. e. sometimes in the air, sometimes in the gas.

Constant interval n between B and D = 1 inch.	Interval p in parts of an inch.			
	When the small ball B was inductive and <i>positive</i> , the discharge was all		When the small ball was inductive and <i>negative</i> , the discharge was all	
	at p in the air before.	at n in the gas after.	at p in the air before.	at n in the gas after.
	$p =$	$p =$	$p =$	$p =$
In Air	0.40	0.50	0.28	0.30
In Nitrogen	0.30	0.65	0.31	0.40
In Oxygen	0.33	0.52	0.27	0.30
In Hydrogen	0.20	0.40	0.22	0.24
In Coal gas	0.20	0.90	0.20	0.27
In Carbonic acid	0.64	1.30	0.30	0.45

1508. These results are the same generally, as far as they go, as those of the like nature in the last series (1388.), and confirm the

conclusion that different gases restrain discharge in very different proportions. They are probably not so good as the former ones, for the glass jar not being varnished, acted irregularly, sometimes taking a certain degree of charge as a non-conductor, and at other times acting as a conductor in the conveyance and derangement of that charge. Another cause of difference in the ratios is, no doubt, the relative sizes of the discharge balls in air; in the former case they were of very different size, here they were alike.

1509. In future experiments intended to have the character of accuracy, the influence of these circumstances ought to be ascertained, and, above all things, the gases themselves ought to be contained in vessels of metal, and not of glass.

1510. The next set of results are those obtained when the intervals n and o (fig. 17.) were made equal to each other, and relate to the greater facility of discharge at the small ball, when rendered positive or negative (1493.).

1511. In *air*, with the intervals = 0.4 of an inch, A and B being inductive and positive, discharge was nearly equal at n and o ; when A and B were inductive and negative, the discharge was mostly at n by negative brush. When the intervals were = 0.8 of an inch, with A and B inductive positively, all discharge was at n by positive brush; with A and B inductive negatively, all the discharge was at n by a negative brush. It is doubtful, therefore, from these results, whether the negative ball has any greater facility than the positive.

1512. *Nitrogen*.—Intervals n and o = 0.4 of an inch: A B inductive positive, discharge at both intervals, most at n , by positive sparks; A B inductive negative, discharge equal at n and o . The intervals made = 0.8 of an inch: A B inductive positive, discharge all at n by positive brush; A B inductive negative, discharge most at o by positive brush. In this gas, therefore, though the difference is not decisive, it would seem that the positive small ball caused the most ready discharge.

1513. *Oxygen*.—Intervals n and o = 0.4 of an inch: A, B inductive positive, discharge nearly equal; inductive negative, discharge mostly at n by negative brush. Made the intervals = 0.8 of an inch: A B inductive positive, discharge at n and o ; inductive negative, discharge all at o by negative brush. So here the negative small ball seems to give the most ready discharge.

1514. *Hydrogen*.—Intervals n and o = 0.4 of an inch: A B inductive positive, discharge nearly equal; inductive negative, discharge mostly at o . Intervals = 0.8 of an inch: A and B inductive positive, discharge mostly at n , as positive brush; inductive negative, discharge mostly at o , as positive brush. Here the positive discharge seems most facile.

1515. *Coal gas*.— n and o = 0.4 of an inch: A B inductive positive, discharge nearly all at o by negative spark: A B inductive negative, discharge nearly all at n by negative spark. Intervals = 0.8 of an inch, and A B inductive positive, discharge mostly at o by negative brush: A B inductive negative, discharge all at n by negative brush. Here the negative discharge most facile.

1516. *Carbonic acid gas*.— n and $o = 0.4$ of an inch : A B inductive positive, discharge nearly all at o , or negative : A B inductive negative, discharge nearly all at n , or negative. Intervals = 0.8 of an inch : A B inductive positive, discharge mostly at o , or negative : A B inductive negative, discharge all at n , or negative. In this case the negative had a decided advantage in facility of discharge.

1517. Thus, if we may trust this form of experiment, the negative small ball has a decided advantage in facilitating disruptive discharge over the positive small ball in some gases, as in carbonic acid gas and coal gas (1399.), whilst in others that conclusion seems more doubtful; and in others, again, there seems a probability that the positive small ball may be superior. All these results were obtained at very nearly the same pressure of the atmosphere.

1518. I made some experiments in these gases whilst in the air jar (fig. 17.), as to the change from spark to brush, analogous to those in the open air already described (1486. 1487.). I will give in a table, the results as to when brush began to appear mingled with the spark; but the after results were so varied, and the nature of the discharge in different gases so different, that to insert the results obtained without further investigation, would be of little use. At intervals less than those expressed the discharge was always by spark.

	Discharge between balls B and D.		Discharge between balls A and C.	
	Small ball B inductive pos	Small ball B inductive neg	Large ball A inductive pos	Large ball A inductive neg
Air	0.55	0.30	0.40	0.75
Nitrogen	0.30	0.40	0.52	0.41
Oxygen	0.70	0.30	0.45	0.82
Hydrogen	0.20	0.10		
Coal gas	0.13	0.30	0.30	0.14
Carbonic acid	0.82	0.43	1.60	{ above 1.80 had not sp.

1519. It is to be understood that sparks occurred at much higher intervals than these; the table only expresses that distance beneath which all discharge was as spark. Some curious relations of the different gases to discharge are already discernible, but it would be useless to consider them until illustrated by further experiments.

1520. I ought not to omit noticing here, that Professor BELLI of Milan has published a very valuable set of experiments on the relative dissipation of positive and negative electricity in the air*; he finds the latter far more ready, in this respect, than the former.

1521. I made some experiments of a similar kind, but with sustained high charges; the results were less striking than those of Signore BELLI, and I did not consider them as satisfactory. I may be allowed to mention, in connection with the subject, an interfering

* Bibliothéque Universelle, 1836, September, p. 152.

effect which embarrassed me for a long time. When I threw positive electricity from a given point into the air, a certain intensity was indicated by an electrometer on the conductor connected with the point, but as the operation continued this intensity rose several degrees; then making the conductor negative with the same point attached to it, and all other things remaining the same, a certain degree of tension was observed in the first instance, which also gradually rose as the operation proceeded. Returning the conductor to the positive state, the tension was at first low, but rose as before; and so also when again made negative.

1522. This result appeared to indicate that the point which had been given of one electricity, was, by that, more fitted for a short time to give of the other. But on closer examination I found the whole depended upon the inductive reaction of that air, which being charged by the point, and gradually increasing in quantity before it, as the positive or negative issue was continued, diverted and removed a part of the inductive action of the surrounding wall, and thus apparently affected the powers of the point, whilst really it was the dielectric itself that was causing the change of tension.

1523. The results connected with the different conditions of positive and negative discharge will have a far greater influence on the philosophy of electrical science than we at present imagine, especially if, as I believe, they depend on the peculiarity and degree of polarized condition which the molecules of the dielectrics concerned acquire (1503. 1600.). Thus, for instance, the relation of our atmosphere and the earth within it, to the occurrence of spark or brush, must be especial and not accidental. It would not else consist with other meteorological phenomena, also of course dependent on the special properties of the air, and which being themselves in harmony the most perfect with the functions of animal and vegetable life, are yet restricted in their actions, not by loose regulations, but by laws the most precise.

1524. Even in the passage through air of the voltaic current, we see the peculiarities of positive and negative discharge at the two charcoal points; and if these discharges are made to take place simultaneously to mercury, the distinction is still more remarkable.

1525. It seems very possible that the striking difference recently observed and described by my friend Professor DANIELL*, namely, that when a zinc and a copper ball, the same in size, were placed respectively in copper and zinc spheres, also the same in size, and excited by electrolytes or dielectrics of the same strength and nature, the zinc ball far surpassed the zinc sphere in action, may also be connected with these phenomena; for it is not difficult to conceive how the polarity of the particles shall be affected by the circumstance of the positive surface, namely the zinc, being the larger or the smaller of the two inclosing the electrolyte. It is even possible, that with different electrolytes or dielectrics the ratio may be considerably varied, or in some cases even inverted.

* Philosophical Transactions, 1838, p. 47.

Glow discharge.

1526. That form of disruptive discharge which appears as a *glow* (1359. 1405.), is very peculiar and beautiful: it seems to depend on a quick and almost continuous charging of the air close to, and in contact with, the conductor.

1527. *Diminution of the charging surface* will produce it. Thus, when a rod 0.3 of an inch in diameter, with a rounded termination, was rendered positive in free air, it gave fine brushes from the extremity, but occasionally these disappeared, and a quiet phosphorescent continuous glow took their place, covering the whole of the end of the wire, and extending a very small distance from the metal into the air. With a rod 0.2 of an inch in diameter the glow was more readily produced. With still smaller rods, and also with blunt conical points, it occurred still more readily; and with a fine point I could not obtain the brush in free air, but only this glow. The positive glow and the positive star are, in fact, the same.

1528. *Increase of power in the machine* tends to produce the glow; for rounded terminations which will give only brushes where the machine is in weak action, will readily give the glow when it is in good order.

1529. *Rarefaction of the air* wonderfully favours the glow phenomena. A brass ball, two and a half inches in diameter, being made positively inductive in an air-pump receiver, became covered with glow over an area of two inches in diameter, when the pressure was reduced to 4.4 inches of mercury. By a little adjustment the ball could be covered all over with this light. Using a brass ball 1.25 inches in diameter, and making it inducteously positive by an inductive negative point, the phenomena, at high degrees of rarefaction, were exceedingly beautiful. The glow came over the positive ball, and gradually increased in brightness, until it was at last very luminous; and it also stood up like a low flame, half an inch or more in height. On touching the sides of the glass jar this lambent flame was affected, assumed a ring form, like a crown on the top of the ball, appeared flexible, and revolved with a comparatively slow motion, i. e. about four or five times in a second. This ring-shape and revolution are beautifully connected with the mechanical currents (1576.) taking place within the receiver. These glows in rarefied air are often highly exalted in beauty by a spark discharge at the conductor (1551. *Note*).

1530. To obtain a *negative glow* in air at common pressures is difficult. I did not procure it on the rod 0.3 of an inch in diameter by my machine, nor on much smaller rods; and it is questionable as yet, whether, even on fine points, what is called the negative star is a very reduced and minute, but still intermitting brush, or a glow similar to that obtained on a positive point.

1531. In rarefied air the negative glow can easily be obtained. If the rounded ends of two metal rods, about 0.2 of an inch in diameter, are introduced into a globe or jar (the air within being rarefied), and being opposite to each other. are about four inches

apart, the glow can be obtained on both rods, covering not only the ends, but an inch or two of the part behind. On using *balls* in the air-pump jar, and adjusting the distance and exhaustion, the negative ball could be covered with glow, whether it were the inductive or the inductive surface.

1532. When rods are used it is necessary to be aware that, if placed concentrically in the jar or globe, the light on one rod is often reflected by the sides of the vessel on to the other rod, and makes it apparently luminous, when really it is not so. This effect may be detected by shifting the eye at the time of observation, or avoided by using blackened rods.

1533. It is curious to observe the relation of glow, brush, and spark to each other, as produced by positive or negative surfaces; thus, beginning with spark discharge, it passes into brush much sooner when the surface at which the discharge commences (1484.) is negative, than it does when positive; but proceeding onwards in the order of change, we find that the positive brush passes into *glow* long before the negative brush does. So that, though each presents the three conditions in the same general order, the series are not precisely the same. It is probable, that, when these points are minutely examined, as they must be shortly, we shall find that each different gas or dielectric presents its own peculiar results, dependent upon the mode in which its particles assume polar electric condition.

1534. The glow occurs in all gases in which I have looked for it. These are air, nitrogen, oxygen, hydrogen, coal gas, carbonic acid, muriatic acid, sulphurous acid and ammonia. I thought also that I obtained it in oil of turpentine, but if so it was very dull and small.

1535. The glow is always accompanied by a wind proceeding either directly out from the glowing part, or directly towards it; the former being the most general case. This takes place even when the glow occurs upon a ball of considerable size: and if matters be so arranged that the ready and regular access of air to a part exhibiting the glow be interfered with or prevented, the glow then disappears.

1536. I have never been able to analyse or separate the glow into visible elementary intermitting discharges (1427. 1433.), nor to obtain the other evidence of intermitting action, namely an audible sound (1431.). The want of success, as respects trials made by ocular means, may depend upon the large size of the glow preventing the separation of the visible images: and, indeed, if it does intermit, it is not likely that all parts intermit at once with a simultaneous regularity.

1537. All the effects tend to show, that *glow* is due to a continuous charge or discharge of air; in the former case being accompanied by a current from, and in the latter by one to, the place of the glow. As the surrounding air comes up to the charged conductor, on attaining that spot at which the tension of the particles is raised to the sufficient degree (1270. 1410.), it becomes charged,

and then moves off, by the joint action of the forces to which it is subject; and, at the same time that it makes way for other particles to come and be charged in turn, actually helps to form that current by which they are brought into the necessary position. Thus, through the regularity of the forces, a constant and quiet result is produced; and that result is, the charging of successive portions of air, the production of a current, and of a continuous glow.

1538. I have frequently been able to make the termination of a rod, which, when left to itself, would produce a brush, produce in preference a glow, simply by aiding the formation of a current of air at its extremity; and, on the other hand, it is not at all difficult to convert the glow into brushes, by affecting the current of air (1574. 1579.) or the inductive action near it.

1539. The transition from glow, on the one hand, to brush and spark, on the other, and, therefore, their connexion, may be established in various ways. Those circumstances which tend to facilitate the charge of the air by the excited conductor, and also those which tend to keep the tension at the same degree notwithstanding the discharge, assist in producing the glow; whereas those which tend to resist the charge of the air or other dielectric, and those which favour the accumulation of electric force prior to discharge, which, sinking by that act, has to be exalted before the tension can again acquire the requisite degree, favour intermitting discharge, and, therefore, the production of brush or spark. Thus, rarefaction of the air, the removal of large conducting surfaces from the neighbourhood of the glowing termination, the presentation of a sharp point towards it, help to sustain or produce the glow: but the condensation of the air, the presentation of the hand or other large surface, the gradual approximation of a discharging ball, tend to convert the glow into brush or even spark. All these circumstances may be traced and reduced, in a manner easily comprehensible, to their relative power of assisting to produce, either a *continuous* discharge to the glow, which gives the air; or an interrupted one, which produces the brush, and, in a more exalted condition, the spark.

1540. The rounded end of a brass rod, 0.3 of an inch in diameter, was covered with a positive glow by the working of an electrical machine: on stopping the machine, so that the charge of the connected conductor should fall, the glow changed for a moment into brushes just before the discharge ceased altogether, illustrating the necessity for a certain high continuous charge, for a certain sized termination. Working the machine so that the intensity should be just low enough to give continual brushes from the end in free air, the approach of a fine point changed these brushes into a glow. Working the machine so that the termination presented a continual glow in free air, the gradual approach of the hand caused the glow to contract at the very end of the wire, then to throw out a

luminous point, which, becoming a foot stalk (1426.), finally produced brushes with large ramifications.

1541. Greasing the end of a rounded wire will immediately make it produce brushes instead of glow. A ball having a blunt point which can be made to project more or less beyond its surface, at pleasure, can be made to produce every gradation from glow, through brush, to spark.

1542. It is also very interesting and instructive to trace the transition from spark to glow, through the intermediate condition of stream, between ends in a vessel containing air more or less rarefied ; but I fear to be prolix.

1543. All the effects show, that the glow is in its nature exactly the same as the luminous part of a brush or ramification, namely a charging of air ; the only difference being, that the glow has a continuous appearance from the constant renewal of the same action in the same place, whereas the ramification is due to a momentary, independent and intermitting action of the same kind.

Dark Discharge.

1544. I will now notice a very remarkable circumstance in the luminous discharge accompanied by negative glow, which may, perhaps, be correctly traced hereafter into discharges of much higher intensity. Two brass rods, 0.3 of an inch in diameter, entering a glass globe on opposite sides, had their ends brought into contact, and the air about them very much rarefied. A discharge of electricity from the machine was then made through them, and whilst that was continued the ends were separated from each other. At the moment of separation a continuous glow came over the end of the negative rod, the positive termination remaining quite dark. As the distance was increased, a purple stream or haze appeared on the end of the positive rod, and proceeded directly outwards towards the negative rod ; elongating as the interval was enlarged, but never joining the negative glow, there being always a short dark space between. This space, of about 1-16th or 1-20th of an inch, was apparently invariable in its extent and its position, relative to the negative rod ; nor did the negative glow vary. Whether the negative end were inductive or inducteous, the same effect was produced. It was strange to see the positive purple haze diminish or lengthen as the ends were separated, and yet this dark space and the negative glow remain unaltered (fig. 19.).

1545. Two balls were then used in a large air-pump receiver, and the air rarefied. The usual transitions in the character of the discharge took place ; but whenever the luminous stream, which appears after the spark and the brush have ceased, was itself changed into glow at the balls, the dark space occurred, and that whether the one or the other ball was made inductive, or positive, or negative.

1546. Sometimes when the negative ball was large, the machine in powerful action, and the rarefaction high, the ball would be

covered over half its surface with glow, and then, upon a hasty observation, would seem to exhibit no dark space: but this was a deception, arising from the overlapping of the convex termination of the negative glow and the concave termination of the positive stream. More careful observation and experiment have convinced me, that when the negative glow occurs it never visibly touches the luminous part of the positive discharge, but that the dark space is always there.

1547. This singular separation of the positive and negative discharge, as far as concerns their luminous character, under circumstances which one would have thought very favourable to their coalescence, is probably connected with their differences when in the form of brush, and is perhaps even dependent on the same cause. Further, there is every likelihood that the dark parts which occur in feeble sparks are also connected with these phenomena*. To understand them would be very important, for it is quite clear that in many of the experiments, indeed in all that I have quoted, discharge is taking place across the dark part of the dielectric to an extent quite equal to what occurs in the luminous part. This difference in the result would seem to imply a distinction in the modes by which the two electric forces are brought into equilibrium in the respective parts; and looking upon all the phenomena as giving additional proofs, that it is to the condition of the particles of the dielectric we must refer for the principles of induction and discharge, so it would be of great importance if we could know accurately in what the difference of action in the dark and the luminous parts consisted.

1548. The dark discharge through air (1552.), which in the case mentioned is very evident (1544.), leads to the enquiry, whether the particles of air are generally capable of affecting discharge from one to another without becoming luminous; and the enquiry is important, because it is connected with that degree of tension which is necessary to originate discharge (1368. 1370.). Discharge between air and conductors without luminous appearances are very common; and non-luminous discharges by carrying currents of air and other fluids (1562. 1595.) are also common enough: but these are not cases in point, for they are not discharges between insulating particles.

1549. An arrangement was made for discharge between two balls (1485. fig. 15., but, in place of connecting the inductive ball directly with the discharging train, it was put in communication with the inside coating of a Leyden jar, and the discharging train with the outside coating. Then working the machine, it was found that whenever sonorous and luminous discharge occurred at the balls A B, the jar became charged; but that when these did not occur, the jar required no charge; and such was the case when small rounded terminations were used¹ in place of the balls, and also in whatever manner they were arranged. Under these circumstances, therefore,

* See Professor JOHNSON's experiments. SILLIMAN's Journal, xxv. p. 57.

discharge even between the air and conductors was always luminous.

1550. But in other cases, the phenomena are such as to make it almost certain, that dark discharge can take place across air. If the rounded end of a metal rod, 0.15 of an inch in diameter, be made to give a good negative brush, the approach of a smaller end or a blunt point opposite to it will, at a certain distance, cause a diminution of the brush, and a glow will appear on the positive inductive wire, accompanied by a current of air passing from it. Now, as the air is being charged both at the positive and negative surfaces, it seems a reasonable conclusion, that the charged portions meet somewhere in the interval, and there discharge to each other, without producing any luminous phenomena. It is possible, however, that the air electrified positively at the glowing end may travel on towards the negative surface, and actually form that atmosphere into which the visible negative brushes dart, in which case dark discharge need not, of necessity occur. But I incline to the former opinion, and think that the diminution in size of the negative brush, as the positive glow comes on to the end of the opposed wire, is in favour of that view.

1551. Using rarefied air at the dielectric, it is very easy to obtain luminous phenomena as brushes, or glow, upon both conducting balls or terminations, whilst the interval is dark, and that, when the action is so momentary that I think we cannot refer to currents as affecting discharge across the dark part. Thus if two balls, about an inch in diameter, and 4 or more inches apart, have the air rarefied about them, and are then interposed in the course of discharge, an interrupted or spark current being produced at the machine,* each termination may be made to show luminous phenomena, whilst more or less of the interval is quite dark. The discharge will pass as suddenly as a retarded spark (295. 334.), i. e. in an interval of time almost inappreciably small, and in such a case, I think it must have passed across the dark part as true disruptive discharge, and not by convection.

1552. Hence I conclude that dark disruptive discharge may occur (1547. 1550.); and also, that, in the luminous brush, the visible ramifications may not show the full extent of the disruptive discharge (1444. 1452.), but that each may have a dark outside, enveloping, as it were, every part through which the discharge extends. It is probable, even, that there are such things as dark discharges analogous in form to the brush and spark, but not luminous in any part (1445.).

1553. The occurrence of dark discharge in any case shows at how low a tension disruptive discharge may occur (1548.), and indicates that the light of the ultimate brush or spark is in no relation to the intensity required (1368. 1370.). So to speak, the discharge begins

* By spark current I mean one passing in a series of spark between the conductor of the machine and the apparatus: by a continuous current one that passes through metallic conductors, and in that respect without interruption at the same place.

in darkness, and the light is a mere consequence of the quantity which, after discharge has commenced, flows to that spot and there finds its most facile passage (1418. 1435.). As an illustration of the growth generally of discharge, I may remark that, in the experiments on the transition in oxygen of the discharge from spark to brush (1518.), every spark was immediately preceded by a short brush.

1554. The phenomena relative to dark discharge in other gases, though differing in certain characters from those in air, confirm the conclusions drawn above. The two rounded terminations (1544.) (fig. 19.), were placed in *muriatic acid gas* (1445. 1463.) at the pressure of 6·5 inches of mercury, and a continuous machine current of electricity sent through the apparatus: bright sparks occurred until the interval was about or above an inch, when they were replaced by squat brushy intermitting glows upon both terminations, with a dark part between. When the current at the machine was in spark, then each spark caused a discharge across the muriatic acid gas, which, with a certain interval, was bright; with a large interval, was straight across and flamy, like a very exhausted and sudden, but not a dense sharp spark; and with a still larger interval, produced a feeble brush on the inductive positive end, and a glow on the inductive negative end, the dark part being between (1544.); and at such times, the spark at the conductor, instead of being sudden and sonorous, was dull and quiet (334.).

1555. On introducing more muriatic acid gas, until the pressure was 29·97 inches, the same terminations gave bright sparks within at small distances; but when they were about an inch or more apart, the discharge was generally with very small brushes and glow, and frequently with no light at all, though electricity had passed through the gas. Whenever the bright spark did pass through the muriatic acid gas at this pressure, it was bright throughout, presenting no dark or dull space.

1556. In *coal gas*, at common pressures, when the distance was about an inch, the discharge was accompanied by short brushes on the ends, and a dark interval of half an inch or more between them, notwithstanding the discharge had the sharp quick sound of a dull spark, and could not have depended in the dark part on *convection*.

1557. This gas presents several curious points in relation to the bright and dark parts of spark discharge. When bright sparks passed between the rod ends 0·3 of an inch in diameter (1544.), very sudden dark parts would occur next to the brightest portions of the spark. Again, with these ends and also with balls (1422.), the bright sparks would be sometimes red, sometimes green, occasionally green and red in different parts of the same spark. Again, in the experiments described (1518.), at certain intervals a very peculiar pale, dull, yet sudden discharge would pass, which, though apparently weak, was very direct in its course, and accompanied by a sharp snapping noise, as if quick in its occurrence.

1558. *Hydrogen* frequently gave peculiar sparks, one part being

bright red, whilst the other was a dull pale gray, or else the whole spark was dull and peculiar.

1559. *Nitrogen* presented a very remarkable discharge, between two balls of the respective diameters of 0.15 and two inches (1506, 1518.), the smaller one being rendered negative either directly or inductively. The peculiar discharge occurred at intervals between 0.42 and 0.68, and even at 1.4 inches when the large ball was inductive positively; it consisted of a little brushy part on the small negative ball, then a dark space, and lastly a dull straight line on the large positive ball (fig. 20.). The position of the dark space was very constant, and is probably in direct relation to the dark space described when negative glow was produced (1544.). When by any circumstance a bright spark was determined, the contrast with the peculiar spark described was very striking; for it always had a faint purple part, but the place of this part was constantly near the positive ball.

1560. Thus dark discharge appears to be decidedly established. But its establishment is accompanied by proofs that it occurs in different degrees and modes in different gases. Hence then another specific action, added to the many (1296. 1398. 1399. 1423. 1454. 1503.) by which the electrical relations of insulating dielectrics are distinguished and established, and another argument in favour of that molecular theory of induction, which is at present under examination*.

1561. What I have had to say regarding the disruptive discharge has extended to some length, but I hope will be excused in consequence of the importance of the subject. Before concluding my remarks, I will again intimate in the form of a query, whether we have not reason to consider the tension or retention and after discharge in air or other insulating dielectrics, as the same thing with retardation and discharge in a metal wire, differing only, but almost infinitely, in degree (1334. 1336.). In other words, can we not, by a gradual chain of association, carry up discharge from its occurrence in air, through spermaceti and water, to solutions, and then on to chlorides, oxides and metals, without any essential change in its character; and at the same time, connecting the insensible conduction of air, through muriatic acid gas and the dark discharge, with the better conduction of spermaceti, water, and the all but perfect conduction of the metals, associate the phenomena at both extremes? and may it not be, that the retardation and ignition of a wire are effects exactly correspondent in their nature to the retention of charge and spark in air? If so, here again the two extremes in property amongst dielectrics will be found to be in intimate relation, the whole difference probably depending upon the mode and degree in which their particles polarize under the influence of inductive actions (1338. 1603. 1610.).

* I cannot resist referring here by a note to EIOR's philosophical view of the nature of the light of the electric discharge, *Annales de Chimie*, liii. p. 321.

¶ x. *Convection ; or carrying discharge.*

1562. The last kind of discharge which I have to consider is that effected by the motion of charged particles from place to place. It is apparently very different in its nature to any of the former modes of discharge (1319.), but, as the result is the same, may be of great importance in illustrating, not merely the nature of discharge itself, but also of what we call the electric current. It often, as before observed, in cases of brush and glow (1440. 1535.), joins its effect to that of disruptive discharge, to complete the act of neutralization amongst the electric forces.

1563. The particles which being charged, then travel, may be either of insulating or conducting matter, large or small. The consideration in the first place of a large particle of conducting matter may perhaps help our conceptions.

1564. A copper boiler 3 feet in diameter was insulated and electrified, but so feebly, that dissipation by brushes or disruptive discharge did not occur at its edges or projecting parts in a sensible degree. A brass ball, 2 inches in diameter, suspended by a clean white silk thread, was brought towards its, and it was found that, if the ball was held for a second or two near any part of the charged surface of the boiler, at such distance (two inches more or less) as not to receive any direct charge from it, it became itself charged, although insulated the whole time; and its electricity was the reverse of that of the boiler.

1565. This effect was the strongest opposite the edges and projecting parts of the boiler, and weaker opposite the sides, or those extended portions of the surface which, according to COULOMB'S results, have the weakest charge. It was very strong opposite a rod projecting a little way from the boiler. It occurred when the copper was charged negatively as well as positively: it was produced also with small balls down to 0.2 of an inch and less in diameter, and also with smaller charged conductors than the copper. It is, indeed, hardly possible in some cases to carry an insulated ball within an inch or two of a charged plane or convex surface without its receiving a charge of the contrary kind to that of the surface.

1566. This effect is one of induction, not of communication. The ball, when related to the positive charged surface by the intervening dielectric, has its opposite sides brought into contrary states, that side towards the boiler being negative and the outer side positive. More inductive action is directed towards it than would have passed across the same place if the ball had not been there, for several reasons; amongst others, because, being a conductor, the resistance of the particles of the dielectric, which otherwise would have been there, is removed (1298.); and also, because the reacting positive surface of the ball being projected further out from the boiler than when there is no introduction of conducting matter, is more free therefore to act through the rest of the dielectric towards surrounding conductors, and so favours the exaltation

of that inductric polarity which is directed in its course. It is, as to the exaltation of force upon its outer surface beyond that upon the inductric surface of the boiler, as if the latter were itself protuberant in that direction. Thus it acquires a state like, but higher than, that of the surface of the boiler which causes it; and sufficiently exalted to discharge at its positive surface to the air, or to affect small particles, as it is itself affected by the boiler, and they flying to it, take a charge and pass off; and so the ball, as a whole, is brought into the contrary inductive state. The consequence is, that, if free to move, its tendency, under the influence of all the forces, to approach the boiler is increased, whilst it at the same time becomes more and more exalted in its condition, both of polarity and charge, until, at a certain distance, discharge takes place, it acquires the same state as the boiler, is repelled, and passing to that conductor most favourably circumstanced to discharge it, there resumes its first indifferent condition.

1567. It seems to me, that the manner in which inductric bodies affect uncharged floating or moveable conductors near them, is very frequently of this nature, and generally so when it ends in a carrying operation (1562. 1602.). The manner in which, whilst the dominant inductric body cannot give off its electricity to the air, the inductive body *can* effect the discharge of the same kind of force, is curious, and, in the case of elongated or irregularly shaped conductors, such as filaments or particles of dust, the effect will often be very ready, and the consequent attraction very immediate.

1568. The effect described is also probably influential in causing those variations in spark discharge referred to in the last series (1386. 1390.): for if a particle of dust were drawn towards the axis of induction between the balls, it would tend, whilst at some distance from that axis, to commence discharge at itself, in the manner described (1566.), and that commencement might so far facilitate the act (1417. 1420.) as to make the complete discharge, as spark, pass through the particle, though it might not be the shortest course from ball to ball. So also, with equal balls at equal distances, as in the experiments of comparison already described (1493. 1506.), a particle being between one pair of balls would cause discharge there in preference; or even if a particle were between each, difference of size or shape would give one for the time a predominance over the other.

1569. The power of particles of dust to carry off electricity in cases of high tension is well known, and I have already mentioned some instances of the kind in the use of the inductive apparatus (1201.). The general operation is very well shown by large light objects, as the toy called the electrical spider; or, if smaller ones are wanted for philosophical investigation, by the smoke of a glowing green wax taper, which, presenting a successive stream of such particles, makes their course visible.

1570. On using oil of turpentine as the dielectric, the action and course of small conducting carrying particles in it can be well ob-

served. A few short pieces of thread will supply the place of carriers, and their progressive action is exceedingly interesting.

1571. A very striking effect was produced on oil of turpentine, which, whether it was due to the carrying power of the particles in it, or to any other action of them, is perhaps as yet doubtful. A portion of that fluid in a glass vessel had a large uninsulated silver dish at the bottom, and an electrified metal rod with a round termination dipping into it at the top. The insulation was very good, and the attraction and other phenomena striking. The rod end, with a drop of gum water attached to it, was then electrified in the fluid; the gum water soon spun off in fine threads, and was quickly dissipated through the oil of turpentine. By the time that four drops had in this way been commingled with a pint of the dielectric, the latter had lost by far the greatest portion of its insulating power; no sparks could be obtained in the fluid; and all the phenomena dependent upon insulation had sunk to a low degree. The fluid was very slightly turbid. Upon being filtered through paper only, it resumed its first clearness, and now insulated as well as before. The water, therefore, was merely diffused through the oil of turpentine, not combined with or dissolved in it: but whether the minute particles acted as carriers, or whether they were not rather gathered together in the line of highest inductive tension (1350.), and there, being drawn into elongated forms by the electric forces, combined their effects to produce a band of matter having considerable conducting power, as compared with the oil of turpentine elsewhere, is as yet questionable.

1572 The analogy between the action of solid conducting carrying particles and that of the charged particles of fluid insulating substances, acting as dielectrics, is very evident and simple; but in the latter case the result is, necessarily, currents in the mobile media. Particles are brought by inductive action into a polar state; and the latter, after rising to a certain tension (1370.), is followed by a communication of a part of the force originally on the conductor; the particles consequently become charged, and then, under the joint influence of the repellent and attractive forces, are urged towards a discharging place, or to that spot where these inductive forces are most easily compensated by the contrary inductive forces.

1573. Why a point should be so exceedingly favourable to the production of currents in a fluid insulating dielectric, as air, is very evident. It is at the extremity of the point that the intensity necessary to charge the air is first acquired (1374.); it is from thence that the charged particle recedes; and the mechanical force which it impresses on the air to form a current, is in every way favoured by the shape and position of the rod, of which the point forms the termination. At the same time, the point, having become the origin of an active mechanical force, does, by the very act of causing that force, namely, by discharge, prevent any other part of the rod from acquiring the same necessary condition, and so preserves and sustains its own predominance.

1574. The very varied and beautiful phenomena produced by

sheltering or enclosing the point, illustrate the production of the current exceedingly well, and justify the same conclusions ; it being remembered that in such cases the effect upon the discharge is of two kinds. For the current may be interfered with by stopping the access of fresh uncharged air, or retarding the removal of that which has been charged, as when a point is electrified in a tube of insulating matter closed at one extremity ; or the *electric condition* of the point itself may be altered by the relation of other parts in its neighbourhood, also rendered electric, as when the point is in a metal tube, by the metal itself, or when it is in the glass tube, by a similar action of the charged parts of the glass, or even by the surrounding air which has been charged, and which cannot escape.

1575. Whenever it is intended to observe such inductive phenomena in a fluid dielectric as have a direct relation to, and dependence upon, the fluidity of the medium, such, for instance, as a discharge from points, or attractions and repulsions, &c., then the mass of the fluid should be great, and in such proportion to the distance between the inductive and inductive surfaces as to include all the *lines of inductive force* (1369.) between them ; otherwise, the effects of currents, attraction, &c., which are the resultants of all these forces, cannot be obtained. The phenomena which occur in the open air, or in the middle of a globe filled with oil of turpentine will not take place in the same media if confined in tubes of glass, shell-lac, sulphur, or other such substances, though they be excellent insulating dielectrics ; nor can they be expected ; for in such cases, the polar forces, instead of being all dispersed amongst fluid particles, which tend to move under their influence, are now associated in many parts with particles that, notwithstanding their tendency to motion, are constrained to remain quiescent.

1576. The varied circumstances under which, with conductors differently formed and constituted, currents can occur, all illustrate the same simplicity of production. A *ball*, if the intensity be raised sufficiently on its surface, and that intensity be greatest on a part consistent with the production of a current of air up to and off from it, will produce the effect like a point (1537.) ; such is the case whenever a glow occurs upon a ball, the current being essential to that phenomenon. If as large a sphere as can well be employed with the production of glow be used, the glow will appear at the place where the current leaves the ball, and that will be the part directly opposite to the connection of the ball and rod which supports it ; but by increasing the tension elsewhere, so as to raise it above the tension upon that spot, which can easily be effected inductively, then the place of the glow and the direction of the current will also change, and pass to that spot which for the time is most favourable for their production (1591.).

1577. For instance, approaching the hand towards the ball will tend to cause brush (1539.), but by increasing the supply of electricity the condition of glow may be preserved ; then on moving the hand about from side to side the position of the glow will very evidently move with it.

1578. A point brought towards a glowing ball would at twelve or fourteen inches distance make the glow break into brush, but when still nearer glow was reproduced, probably dependent upon the discharge of wind or air passing from the point to the ball, and this glow was very obedient to the motion of the point, following it in every direction.

1579. Even a current of wind could affect the place of the glow ; for a varnished glass tube being directed sideways towards the ball, air was sometimes blown through it at the ball, and sometimes not. In the former case, the place of the glow was changed a little, as if it were blown away by the current, and this is just the result which might have been anticipated. All these effects illustrate beautifully the general causes and relations, both of the glow and the current of air accompanying it (1574.)

1580 Flame facilitates the production of a current in the dielectric surrounding it. Thus, if a ball which would not occasion a current in the air have a flame, whether large or small, formed on its surface, the current is produced with the greatest ease ; but not the least difficulty can occur in comprehending the effective action of the flame in this case, if its relation, as part of the surrounding dielectric, to the electrified ball, be but for a moment considered (1375. 1380.)

1581. Conducting fluid terminations, instead of rigid points, illustrate in a very beautiful manner the formation of the currents, with their effects and influence in exalting the conditions under which they were commenced. Let the rounded end of a brass rod, 0.2 of an inch or thereabouts in diameter, point downwards in free air ; let it be amalgamated, and have a drop of mercury suspended from it ; and then let it be powerfully electrized. The mercury will present the phenomenon of *glom* ; a current of air will rush along the rod, and set off from the mercury directly downwards ; and the form of the metallic drop will be slightly affected, the convexity at a small part near the middle and lower part becoming greater, whilst it diminishes all round at places a little removed from this spot. The change is from the form of *a* (fig. 21.) to that of *b*, and is due almost, if not entirely, to the mechanical force of the current of air sweeping over its surface.

1582. As a comparative observation, let it be noticed, that a ball gradually brought towards it converts the glow into brushes, and ultimately sparks pass from the most projecting part of the mercury. A point does the same, but at much smaller distances.

1583. Take next a drop of strong solution of muriate of lime ; being electrified, a part will probably be dissipated, but a considerable portion, if the electricity be not too powerful, will remain, forming a conical drop (fig. 22.), accompanied by a strong current. If glow be produced, the drop will be smooth on the surface. If a short low brush is formed, a minute tremulous motion of the liquid will be visible ; but both effects coincide with the principal one to be observed, namely, the regular and continuous charge of air, the formation of a wind or current, and the form given by that

current to the fluid drop. If a discharge ball be gradually brought towards the cone, sparks will at last pass, and these will be from the apex of the cone to the approached ball, indicating a considerable degree of conducting power in this fluid.

1584. With a drop of water, the effects were of the same kind, and were best obtained when a portion of gum water or of syrup hung from a ball (fig. 23.). When the machine was worked slowly, a fine large quiet conical drop, with concave lateral outline, and a small rounded end, was produced, on which the glow appeared, whilst a steady wind issued, in a direction from the point of the cone, of sufficient force to depress the surface of uninsulated water held opposite to the termination. When the machine was worked more rapidly some of the water was driven off; the smaller pointed portion left was roughish on the surface, and the sound of successive brush discharges was heard. With still more electricity, more water was dispersed; that which remained was elongated and contracted, with an alternating motion; a stronger brush discharge was heard, and the vibrations of the water and the successive discharges of the individual brushes were simultaneous. When water from beneath was brought towards the drop, it did not indicate the same regular strong contracted current of air as before; and when the distance was such that sparks passed, the water beneath was *attracted* rather than driven away, and the current of air *ceased*.

1585. When the discharging ball was brought near the drop in its first quiet glowing state (1582.), it converted that glow into brushes, and caused the vibrating motion of the drop. When still nearer, sparks passed, but they were always from the metal of the rod, over the surface of the water, to the point, and then across the air to the ball. This is a natural consequence of the deficient conducting power of the fluid (1584. 1585.).

1586. Why the drop vibrated, changing its form between the periods of discharging brushes, so as to be more or less acute at particular instants, to be most acute when the brush issued forth, and to be isochronous in its action, and how the quiet glowing liquid drop, on assuming the conical form, facilitated, as it were, the first action, are points, as to theory, so evident, that I will not stop to speak of them. The principal thing to observe at present is, the formation of the carrying current of air, and the manner in which it exhibits its existence and influence by giving form to the drop.

1587. That the drop, when of water, or a better conductor than water, is formed into a cone principally by the current of air, is shown amongst other ways (1504.) thus. A sharp point being held opposite the conical drop, the latter soon lost its pointed form; was retracted and became round; the current of air from it ceased, and was replaced by one from the point beneath, which, if the latter were held near enough to the drop, actually blew it aside, and rendered it concave in form.

1588. It is hardly necessary to say what happened with still worse conductors than water, as oil, or oil of turpentine; the fluid itself

was then spun out into threads and carried off, not only because the air rushing over its surface, helped to sweep it away, but also because its insulating particles assumed the same charged state as the particles of air, and, not being able to discharge to them in a greater degree than the air particles themselves could do, were carried off by the same causes which urged these in their course. A similar effect with melted sealing-wax on a metal point forms an old and well-known experiment.

1589. A drop of gum water in the exhausted receiver of the air-pump was not sensibly affected in its form when electrified. When air was let in, it began to show change of shape when the pressure was ten inches of mercury. At the pressure of fourteen or fifteen inches the change was more sensible, and as the air increased in density the effects increased, until they were the same as those in the open atmosphere. The diminished effect in the rare air I refer to the relative diminished energy of its current; that diminution depending, in the first place, on the lower electric condition of the electrified ball in the rarefied medium, and in the next, on the attenuated condition of the dielectric, the cohesive force of water in relation to rarefied air being something like that of mercury to dense air (1581.), whilst that of water in dense air may be compared to that of mercury in oil of turpentine (1597.).

1590. When a ball is covered with a thick conducting fluid, as treacle or syrup, it is easy by inductive action to determine the wind from almost any part of it (1577.); the experiment, which before was of rather difficult performance, being rendered facile in consequence of the fluid enabling that part, which at first was feeble in its action, to rise into an exalted condition by assuming a pointed form.

1591. To produce the current, the electric intensity must rise and continue at *one spot*, namely, at the origin of the current, higher than elsewhere, and then, air having a uniform and ready access, the current is produced. If no current be allowed (1574.), then discharge may take place by brush or spark. But whether it be by brush or spark, or wind, it seems very probable that the initial intensity or tension at which a particle of a given gaseous dielectric charges, or commences discharge, is, under the conditions before expressed, always the same (1410.).

1592. It is not supposed that all the air which enters into motion is electrified; on the contrary, much that is not charged is carried on into the stream. The part which is really charged may be but a small proportion of that which is ultimately set in motion (1442.).

1593. When a drop of gum water (1584.) is made *negative*, it presents a larger cone than when made positive; less of the fluid is thrown off, and yet, when a ball is approached, sparks can hardly be obtained, so pointed is the cone, and so free the discharge. A point held opposite to it did not cause the retraction of the cone to such an extent as when it was positive. All the effects are so different from those presented by the positive cone, that I have no

doubt such drops would present a very instructive method of investigating the difference of positive and negative discharge in air and other dielectrics (1480. 1501.).

1594. That I may not be misunderstood (1587.), I must observe here that I do not consider the cones produced as the result *only* of the current of air or other insulating dielectric over their surface. When the drop is of badly conducting matter, a part of the effect is due to the electrified state of the particles, and this part constitutes almost the whole when the matter is sealing-wax, oil of turpentine, and similar insulating bodies (1586.). But even when the drop is of good conducting matter, as water, solutions, or mercury, though the effect above spoken of will then be insensible (1607.), still it is not the mere current of air or other dielectric which produces all the change of form; for a part is due to those attractive forces by which the charged drop, if free to move, would travel along the line of strongest induction, and not being free to move, has its form elongated until the *sum* of the different forces tending to produce this form is balanced by the cohesive attraction of the fluid. The effect of the attractive forces are well shown when treacle, gum water, or syrup is used; for the long threads which spin out, at the same time that they form the axes of the currents of air, which may still be considered as determined at their points, are like flexible conductors, and show by their directions in what way the attractive forces draw them.

1595. When the phenomena of currents are observed in dense insulating dielectrics, they present us with extraordinary degrees of mechanical force. Thus, if a pint of well rectified and filtered (1571.) oil of turpentine be put into a glass vessel, and two wires be dipped into it in different places, one leading to the electrical machine, and the other to the discharging train, on working the machine the fluid will be thrown into violent motion throughout its whole mass, whilst at the same time it will rise two, three, or four inches up the machine wire, and dart off in jets from it into the air.

1596. If very clean uninsulated mercury be at the bottom of the fluid, and the wire from the machine be terminated either by a ball or a point, and also pass through a glass tube extending both above and below the surface of the oil of turpentine, the currents can be better observed, and will be seen to rush down the wire, proceeding directly from it towards the mercury, and there, diverging in all directions, will ripple its surface strongly, and mounting up at the sides of the vessel, will return to re-enter upon their course.

1597. A drop of mercury being suspended from an amalgamated brass ball, preserved its form almost unchanged in air (1581.); but when immersed in the oil of turpentine it became very pointed, and even particles of the metal could be spun out and carried off by the currents of the dielectric. The form of the liquid metal was just like that of the syrup in air (1584.), the point of the cone being quite as fine, though not so long. By bringing a sharp uninsulated point towards it, it could also be effected in the same manner as the

syrup drop in air (1587.), though not so readily, because of the density and limited quantity of the dielectric.

1598. If the mercury at the bottom of the fluid be connected with the electrical machine, whilst a rod is held in the hand terminating in a ball three-quarters of an inch, less or more, in diameter, and the ball be dipped into the electrified fluid, very striking appearances ensue. When the ball is raised again so as to be at a level nearly out of the fluid, large portions of the latter will seem to cling to it (fig. 24.). If it be raised higher, a column of the oil of turpentine will still connect it with that in the basin below (fig. 25.). If the machine be excited into more powerful action, this will become more bulky, and may then also be raised higher, assuming the form fig. 26; and all the time that these effects continue, currents and counter-currents, sometimes running very close together, may be observed in the raised column of fluid.

1599. It is very difficult to decide by sight the direction of the currents in such experiments as these. If particles of silk are introduced they cling about the conductors; but using drops of water and mercury the course of the fluid dielectric seems well indicated. Thus, if a drop of water be placed at the end of a rod (1571.) over the uninsulated mercury, it is soon swept away in particles streaming downwards towards the mercury. If another drop be placed on the mercury beneath the end of the rod, it is quickly dispersed in all directions in the form of streaming particles, the attractive forces drawing it into elongated portions, and the currents carrying them away. If a drop of mercury be hung from a ball used to raise a column of the fluid (1598.), then the shape of the drop seems to show currents travelling in the fluid in the direction indicated by the arrows (fig. 27.)

1600. A very remarkable effect is produced on these phenomena, connected with positive and negative charge and discharge, namely, that a ball charged positively raises a much higher and larger column of the oil of turpentine than when charged negatively. There can be no doubt that this is connected with the difference of positive and negative action already spoken of (1480. 1525.), and tends much to strengthen the idea that such difference is referable to the particles of the dielectric rather than to the charged conductors, and is dependent upon the mode in which these particles polarize (1503. 1523.).

1601. Whenever currents travel in insulating dielectrics they really effect discharge; and it is important to observe, though a very natural result, that it is indifferent which way the current or particles travel, as with reversed direction their state is reversed. The change is easily made, either in air or oil of turpentine, between two opposed and related rods, for an insulated ball being placed in connection with either rod and brought near its extremity, will cause the current to set towards it from the opposite end.

1602. The two currents often occur at once, as when both terminations present brushes, and frequently when they exhibit the

glow (1531.). In such cases, the charged particles, or many of them, meet and mutually discharge each other (1548. 1612.). If a smoking wax taper be held at the end of an insulating rod towards a charged prime conductor, it will very often happen that two currents will form, and be rendered visible by its vapour, one passing as a fine filament of smoky particles directly to the charged conductor, and the other passing as directly from the same taper wick outwards, and from the conductor; the principles of inductive action and charge, which were referred to in considering the relation of a carrier ball and a conductor (1566.), being here also called into play.

1603. The general analogy, and I think I may say, identity of action found to exist as to insulation and conduction (1338. 1561.) when bodies, the best and the worst in the classes of insulators or conductors, were compared, led me to believe that the phenomena of *convection* in badly conducting media were not without their parallel amongst the best conductors, such even as the metals. Upon consideration, the cones produced by DAVY* in fluid metals, as mercury and tin, seemed to be cases in point, and probably also the elongation of the metallic medium through which a current of electricity was passing, described by AMPERE;† for it is not difficult to conceive, that the diminution of convective effect, consequent upon the high conducting power of the metallic media used in these experiments, might be fully compensated for by the enormous quantity of electricity passing. In fact, it is impossible not to expect *some* effect, whether sensible or not, of the kind in question, when such a current is passing through a fluid offering a sensible resistance to the passage of the electricity, and, thereby, giving proof of a certain degree of insulating power (1328.).

1604. I endeavoured to connect the convective currents in air, oil of turpentine, &c. and those in metals, by intermediate cases, but found this not easy to do. On taking bodies, for instance, which, like water, acids, solutions, fused salts or chlorides, &c., have intermediate conducting powers, the minute quantity of electricity which the common machine can supply (371. 861.) is exhausted instantly, so that the cause of the phenomenon is kept either very low in intensity, or the instant of time during which the effect lasts is so small, that one cannot hope to observe the result sought for. If a voltaic battery be used, these bodies are all electrolytes, and the evolution of gas, or the production of other changes, interferes and prevents observation of the effect required.

1605. There are, nevertheless, some experiments which illustrate the connection. Two platina wires, forming the electrodes of a powerful voltaic battery, were placed side by side, near each other, in distilled water, hermetically sealed up in a strong glass tube, some minute filaments being present in the water. When, from the

* Philosophical Transactions, 1823, p. 155.

† Bibliotheque Universelle, xxi, 47.

evolution of gas and the consequent increased pressure, the bubbles formed on the electrodes were so small as to produce but feebly ascending currents, then it could be observed that the filaments present were attracted and repelled between the two wires, as they would have been between two oppositely charged surfaces in air or oil of turpentine, moving so quickly as to displace and disturb the bubbles and the currents which these tended to form. Now I think it cannot be doubted that under similar circumstances, and with an abundant supply of electricity, of sufficient tension also, convective currents might have been formed; the attractions and repulsions of the filaments were, in fact, the elements of such currents (1572.), and therefore water, though almost infinitely above air or oil of turpentine as a conductor, is a medium in which similar currents can take place.

1606. I had an apparatus made (fig. 28.) in which *a* is a plate of shell-lac, *b* a fine platina wire passing through it, and having only the section of the wire exposed above; *c* a ring of bibulous paper resting on the shell-lac, and *d* distilled water retained by the paper in its place, and just sufficient in quantity to cover the end of the wire *b*; another wire, *e*, touched a piece of tin foil lying in the water, and was also connected with a discharging train; in this way it was easy, by rendering *b* either positive or negative, to send a current of electricity by its extremity into the fluid, and so away by the wire *e*.

1607. On connecting *b* with the conductor of a powerful electrical machine, not the least disturbance of the level of the fluid over the end of the wire during the working of the machine could be observed; but at the same time there was not the smallest indication of electrical charge about the conductor of the machine, so complete was the discharge. I conclude that the quantity of electricity passed in a *given time* had been too small, when compared with the conducting power of the fluid, to produce the desired effect.

1608. I then charged a large Leyden battery (291.), and discharged it through the wire *b*, interposing, however, a wet thread, two feet long, to prevent a spark in the water, and to reduce what would else have been a sudden violent discharge into one of more moderate character, enduring for a sensible length of time (334.). I now did obtain a very brief elevation of the water over the end of the wire; and though a few minute bubbles of gas were at the same time formed there, so as to prevent me from asserting that the effect was unequivocally the same as that obtained by DAVY in the metals, yet, according to my best judgment, it was partly, and I believe principally, of that nature.

1609. I employed a voltaic battery of 100 pair of four-inch plates for experiments of a similar nature with electrolytes. In these cases the shell-lac was cupped, and the wire *b* 0.2 of an inch in diameter. Sometimes I used a positive amalgamated zinc wire in contact with dilute sulphuric acid; at others, a negative copper wire in a solution of sulphate of copper; but, because of the

evolution of gas, the precipitation of copper, &c., I was not able to obtain decided results. It is but right to mention, that when I made use of mercury, endeavouring to repeat DAVY's experiment, the battery of 100 pair was not sufficient to produce the elevations.*

(To be continued.)

PROFESSOR FUCHS on the Analyses of Iron Ores, from the *Gelchrte Anzeigen der bayer. Akademie der Wissenschaften*, Nos. 102—104. 1839.

Description of a simple process for determining the quantity of metal contained in Ores of Iron and other Ferruginous Bodies, and which process also indicates the relative proportions in which the protoxide and the peroxide of that metal occur.

Since we have learnt the way of separating with the greatest nicety the peroxide of iron from the protoxide by the employment of carbonate of lime or carbonate of barytes, it becomes in many cases a matter of no difficulty to determine the respective proportions in which these two oxides occur when met with in combination with other substances.†

There are, however, cases in which we cannot have recourse to the above mentioned method, as for instance, when phosphoric acid is present, and this was the position in which I found myself lately, on having a mineral from Rabenstein to examine, and in which a preliminary quantitative analysis had established the presence of phosphoric acid, and of the protoxide and peroxide of iron.

And as the methods usually resorted to in this case, are complicated and did not appear to me to be sufficiently exact, I set about trying to discover some other method at once simpler and more certain, and my endeavours have, I trust, been successful.

I will now proceed to describe this method at length, remarking only that its use is by no means confined to the case in question, but that it is applicable to the determination of the amount of iron contained in ferruginous bodies in general, and I will then add a word or two concerning the above mentioned mineral from Rabenstein.

The proceeding in question is based on this, namely, that, *when the air is excluded, muriatic acid is quite incapable of dissolving copper, but that on the addition of oxide of iron, or when that substance is*

* In the experiments at the Royal Institution, Sir H. DAVY used, I think, 500 or 600 pairs of plates. Those at the London Institution were made with the apparatus of Mr. PEPYS, (consisting of an enormous single pair of plates), described in the *Philosophical Transactions* for 1823, p. 187.

† For a detailed account of this application of the Carbonate of lime or of barytes, see *Nenes Jahrb. der Ch. und Phys.* Jahrg. 1831. Band II. S. 184.

already at the outset contained in the mixture, the muriatic acid does dissolve a quantity of copper corresponding thereto.

In consequence of this, there is formed on the one hand, protomuriate of iron, or in other words protochloride of iron, and, on the other protomuriate or rather protochloride of copper.

Now supposing that we take a certain quantity of copper, whose weight is accurately determined, and put it into a solution of muriate of iron, and boil it well therein until no more copper is taken up, and then pour off the fluid and weigh the undissolved copper, after having carefully cleansed and dried it, we shall ascertain how much copper has been dissolved.

And this is all we require for ascertaining the quantity of oxide of iron dissolved in the muriatic acid, inasmuch as we have merely to multiply the quantity of copper taken up by the equivalent of peroxide of iron ($= 40$), and then to divide the product by the equivalent of copper (31.7). The quotient indicates the quantity of peroxide existing in the solution.

In other words, the equivalent of copper is to that of the peroxide of iron, as the quantity of copper dissolved is to x , that is to say, to the quantity of peroxide of iron we wish to ascertain.

If we want to learn the amount of metallic iron corresponding to the peroxide, all we have to do is to substitute the equivalent of the metal for that of the peroxide. The calculation is to be made as has been just described.

When the peroxide and the protoxide occur together in the same substance, two experiments are required to determine the quantities of them both. In one experiment, the solution in muriatic acid must have the copper boiled well up in it as has been described, by which means we arrive at the quantity of peroxide which the solution contains. The first step to be taken in proceeding to the second experiment is, to raise the whole of the protoxide present to the higher grade of oxidation, upon which, speaking in a general way, the above process is to be repeated. From the entire quantity of peroxide thus obtained at the conclusion of this second experiment must be deducted what the first experiment gave us, and the remainder is to be reduced to the protoxide by calculation.

I will now describe the course of operations we are to pursue, and the precautions we must observe to perform the experiments with precision, and thus obtain results on whose accuracy we may rely.

1. With respect to the copper we employ in our investigations. Care must be taken that it is in a state of purity, and more especially free from iron. It is, therefore, advisable to prepare the metal oneself by precipitating it from a solution of the sulphate with iron, and then boiling this precipitate up with muriatic acid.

The metal thus obtained is to be melted and rolled into plates, which must be cut into strips 3 or 4 lines broad. Previous to using these strips they should be boiled afresh in muriatic acid, inasmuch as some portion of the proto-muriate adheres to them, the presence of which would naturally interfere with the accuracy of our experiments.

We can easily estimate about how much metal any experiment will require, (a considerable quantity being always to be left after the

We are thus put in possession of a method of analysing iron ores in the moist way which, as far as exactness is concerned, is not inferior to the best method of proceeding in the dry way,—the process is not expensive, neither is any great time required to conduct it, inasmuch as, when all the necessary preparations are duly made, the whole may be, without difficulty, performed within two hours. And this process is not alone confined to the analysis of ores of iron, it may be had recourse to to ascertain how much metallic iron is actually contained in cast-iron or iron of other kinds, and it is likewise applicable to comparing one sort of iron with another.

By means of iron the copper may be precipitated from the solution which has been poured off as above described, and the copper thus obtained is from its purity fitted for further experiments, or we can throw down the copper and iron in combination by means of the usual precipitant and then proceed to test the solution for other substances which we suspect are contained in the iron or iron ore which we are engaged in operating upon.

I will now proceed to describe a few experiments which I instituted, simply with a view of testing the accuracy of the process in question. Chemically pure iron was that which best answered this purpose, for on dissolving this in muriatic acid and raising the solution to the highest grade of oxidation, the quantity of copper taken up would then indicate, if not exactly, at least within a trifle, the amount of iron employed in the experiment. As we however, never fall in with any iron that is, strictly speaking, pure, we must in such enquiries, content ourselves with such as judging from its physical characters, appears to contain but very few impurities.

And if the results which this proceeding then gives coincide with what our former experience has established, and if, theoretically speaking, no objections can be raised against it, we may look on it as fairly borne out, and we shall perhaps be led to give it the preference over many other methods of ascertaining the metallic contents of ores of iron.

My first experiments were made on various kinds of malleable iron, and I subsequently examined other sorts. I will here cite merely a few by way of examples.

Ex. 1. 50 grains of very soft English iron were dissolved in muriatic acid, and then raised to the highest stage of oxidation, by means of chlorate of potash. Into this solution were then inserted 85,8 grains of pure copper, of which there remained 29,6 grains undissolved, 56,2 grains had therefore been taken up. Calculation of the amount of iron, $31,7 : 28 :: 56,2 : x = 49,46$, that is to say, 98,92 per cent of pure iron, a repetition of this experiment gave 99,19.

Ex. 2. 50 grains of piano-wires were treated, speaking in a general way, like the iron above; but the solution was raised to the highest stage of oxidation, by means of a current of chlorine. The copper taken up, amounted to 55,9 grains, corresponding to 49,375 of pure iron. The actual amount of iron was therefore 98,75 per cent.

During the process of solution in muriatic acid, there was a considerable deposit of carbon formed, this however, entirely disappeared, while he chlorine passed through the liquor.

Ex. 3. Grey and white cast-iron, from the furnace in the neighbourhood of Bergen.

50 grains of this iron were treated with hydrochloric acid, chlorate of potash, and the addition of 80 grains of copper, as described in experiment No. 1.

The copper taken up amounted to 53.4 grains, corresponding to 47.16 grains of pure iron, as seen by the calculation below, viz.:

$$28 \times 53.4$$

$$\frac{\quad}{31.4} = 47.16 = 94.33 \text{ per cent.}$$

I found that the foreign substances contained in this iron were carbon, silicon, phosphorus, and sulphur; and that in the following proportions:

Carbon.....	3.43
Silicon.....	1.75
Phosphorus.....	0.37
Sulphur	0.12
Iron	94.33

100.00

The silicon separated itself at the commencement of the operation, while the solution in the hydrochloric acid was going on in the form of silica, and was associated with a certain quantity of carbon, (1.8 per cent. graphite*) and was parted from the latter by means of potash. In a subsequent stage of the process, however, my attention was again directed to it, that is to say—when I came to ascertain the amount of carbon, an operation that I will concisely describe in passing.

For this purpose I had recourse to the per-muriate of iron. A solution of this substance, especially when concentrated, acts with great energy upon pulverised iron, so much so, indeed, that the temperature rises nearly to the boiling point, while a considerable quantity of hydrogen is given off, holding, to some extent, carbon in solution.

Now this, it is clear, would not put us in the way of attaining what we have in view, and hence it becomes necessary to moderate the energy of the agent we employ. And I brought this about by adding carbonate of lime to a moderately strong solution, till the latter assumed a dark brown colour, and a portion of oxide of iron began to be thrown down. This solution in which the hydrochloric acid was neutralised as near as might be, was then poured on to the pulverised iron under examination, and allowed to digest for three days at a gentle heat, being frequently stirred during that interval; by which means the carbon was separated without any sensible portion thereof being carried off in a gaseous form. There was also thrown down, at the same time, a considerable muddy deposit of hydrous oxide of iron.

When there was no longer any metallic iron to be seen, the fluid was poured off; the muddy deposit removed with muriatic acid, and that which remained of a carbonaceous nature was placed in a filter whose weight was known and examined by the usual methods.

* Plumbago.

To ascertain the amount of phosphorus and sulphur, the copper and the iron were thrown down together (as has been above described) from the solution obtained for determining the amount of iron; the phosphoric acid was then precipitated with muriate of lime, and, finally, the sulphuric acid thrown down with muriate of barytes. How the process was carried on further it is not necessary to describe.

Ex. 4. Crystallized carbonate of iron, from Lobenstein.

70 grains were dissolved in muriatic acid, converted to the permuriate of iron with chlorate of potash, and duly boiled up with 60 grains of copper, of which 35.08 grains were taken up.

Hence we calculate the following amounts of protoxide of iron and of metallic iron:

a. $31,7 : 40 :: 35,08 : x = 44,26$ grains of the peroxide.

b. $31,7 : 36 :: 35,08 : x = 39,83$ grains of the protoxide.

c. $31,7 : 28 :: 35,08 : x = 30,98$ grains of metallic iron.

Consequently in 100 parts of this carbonate of iron there are, as a simple calculation shows, 56,9 parts of protoxide of iron, corresponding to 44,3 of metallic iron, and 91,68 of the carbonate of the protoxide. That which is wanting to make up 100, that is to say, 8,32 consists of a carbonate of the protoxide of manganese, and probably, also, of the carbonates of lime and magnesia.

I examined, likewise, this carbonate of iron after being calcined, and which then, as is well known, consists of a mixture of the protoxide and the peroxide of iron. I did not, however, obtain the result I hoped for, that is to say, that the actual composition of this mineral would coincide with that of magnetic iron ore. It contains, however, much more of the protoxide, and less of the peroxide.

Ex. 5. Specular iron ore, from Gleissing, in the Fichtelgebirg.

70 grains were dissolved in muriatic acid, a portion of chlorate of potash being added to the solution.

During the process of solution there was deposited a sandy powder, consisting of nothing but quartz, and weighing 5.2 grains. It was not collected together, however, till the close of the operation. Of 80 grains of copper, with which the fluid was boiled up, there were 51,2 taken up, corresponding to 64.6 grains of peroxide of iron, as the calculation below shows:

$$40 \times 51,2$$

$$\hline = 64.6 \text{ perox. iron.}$$

$$31,7 = 45,22 \text{ metallic iron.}$$

The peroxide of iron and the quartz together make up 69,8 grains: there is a loss, therefore, of only 0.2 grains.

Hence in 100 parts of this specular iron ore we have 92,3 parts of the peroxide of iron, and 7,43 quartz; in other words, it contains 64.7 per cent. metallic iron.

In a second experiment, whose results, in a general way, agreed with the one above, I obtained only 5,46 per cent. of quartz. This latter substance must, therefore, be very unequally distributed in the specular iron ore in question. At all events, it is clear that none of the silica is chemically combined with the peroxide of iron.

The employment of chlorate of potash in this experiment may per-

haps appear superfluous, inasmuch as the iron contained in the one in question is in the state of the red oxide; but I had recourse to it because I observed that the specimen I purposed examining, to a certain extent, affected the magnetic needle, and, therefore, there was ground to suspect the presence of a certain portion of the protoxide. This remark applies also even to several varieties of clay iron ore; and, therefore, when we are desirous of arriving correctly at the quantity of iron they contain, it is advisable to employ a certain portion of chlorate of potash in their analysis.

Ex. 6. Crystallized magnetic iron ore.

If we wish to determine not only the quantity of iron contained in this mineral, but likewise to ascertain the relative proportions in which it occurs in the peroxide and the protoxide, two experiments must be instituted, as we have already said.

1st. Experiment.

The solution in hydrochloric acid, 50 grains of the mineral being taken for the purpose, was treated with chlorate of potash as described above, and was then boiled up with 50 grains of copper, of which 40.71 were taken up.

The entire quantity of peroxide amounts, therefore, to—

$$\frac{40 \times 40.71}{31.7} = 51.36 \text{ grains} = 102.72 \text{ per cent.}$$

In other words, 71.91 per cent, metallic iron.

2nd Experiment.

For this experiment 50 grains were also used, and on their being dissolved in hydrochloric acid, they were at once boiled up with 50 grains of copper without the addition of chlorate of potash, in order to ascertain the quantity of peroxide the mineral contained.

The copper taken up amounted to 27.1 grains; and, therefore, the quantity of peroxide corresponding thereto is,—

$$\frac{40 \times 27.1}{31.7} = 34.2 \text{ grains} = 64.8 \text{ per cent.}$$

Now subtracting this originally existing quantity of peroxide (68.4) from the total amount which we obtained by Experiments 1, (102.72) we have 34.32, corresponding to—

$$\frac{36 \times 34.32}{40} = 30.88 \text{ protoxide.}$$

And this magnetic iron ore, therefore, agreeing very nearly with the formula which Berzelius gives for it, namely, $\frac{1}{2} \text{ Fe}$, contains, in a hundred parts—

Peroxide	68.40
Protoxide	30.88
	<hr/>
	99.28
Loss72
	<hr/>
	100.00

In pursuing the method of analysis here described, our attention must be equally directed to the correctness of the calculation and to exactness of manipulation, otherwise no reliance can be placed on the results. It is moreover indispensably requisite that the equivalents employed in the calculations should be perfectly correct. Berzelius, calling oxygen 100, takes 395.695 as the elementary number of copper, and calling the double atom of hydrogen 1, this agrees very nearly with 31.7, which is the number I have employed.

Berzelius, in the oxygen scale, makes iron 339.213, which very nearly agrees with 27.18 in the hydrogen scale. This number, however, appears somewhat too low, for if we use it in our calculations we fall considerably short of the actual or probable amount of iron in the bodies under review.

And having no reason to call the correctness of the method I here describe in doubt, I have herein followed those who make its elementary number 28, and the results we thereby attain bear us out therein as completely as we can wish.

Were we to call iron 27.5 and copper 31.5 we should perhaps come nearest the truth; but I by no means assert that those numbers are really the right ones.*

I now proceed to the analysis of the phosphate of iron from Rabenstein, which first directed my attention to the above experiments. I have already stated the mineral in question contained peroxide and protoxide of iron, and also phosphoric acid; and a further examination shewed the presence of protoxide of manganese and from 9 to 10 per cent. of water, together with a small portion of phosphate of lime,—this latter substance being probably only an accidental admixture.

As I could not come across any specimens of this mineral but what were impure from foreign admixture, or more or less weathered, I have not yet succeeded in determining its chemical constitution.

The quantity of iron, therefore, that I obtained in three separate experiments varied very considerably; but, at all events, it is clear that the quantity of peroxide exceeds very considerably that of the

It may not be here perhaps out of place to observe, that this method of proceeding (as must, indeed, be evident, on considering the matter for an instant) is, in many instances, applicable to the determination of the amount of copper contained in ores of that metal. For this purpose, the cupreous body in question is to be dissolved in muriatic acid, care being taken that the whole of the copper is converted to the oxide or chloride.

The solution is then, the necessary precautions being duly observed, to be boiled up with copper till it assumes a pale olive-green tint, and becomes colourless on being diluted with water.

It is evident that if no oxide of iron is present, precisely the same quantity of copper will be transferred to the solution as was originally contained therein; and hence we have simply to subtract the quantity of reguline copper remaining from the quantity employed in the experiment in order to ascertain the amount of metal contained in the cupreous substance we have dissolved.

On duly treating, for instance, with metallic copper, a solution for which we employed 100 grains of pure malachite, and which, as we are well aware, contains 57.5 grains of copper, it will be found that, if not precisely yet within a very little, the same quantity of copper will be dissolved therein as the malachite contained. Should the quantity dissolved prove considerably less, it will be a proof that the malachite employed was not in a state of purity.

protoxide. From a small specimen which was apparently tolerably pure and fresh, I obtained, on an examination with copper, 38.9 per cent. of the peroxide of iron, and of the protoxide only 3.87 per cent.

I found, in one analysis, 25.52 per cent. phosphoric acid, upon another occasion as much as 30.27 per cent.

It dissolves easily in hydrochloric acid with the application of warmth, and the colour of the solution is red, like that of the peroxide of iron; it gives, however, a considerable blue precipitate with ferrocyanide of potash.

Before the blowpipe, it melts easily to a blackish grey bead, which, however, scarcely affects the magnet.

What, however, is very characteristic in this mineral is, that in masses, when fresh, it has a dark greenish black colour, while its streak is of a yellowish green. Not unfrequently, however, it is partially, and occasionally, indeed, throughout the entire mass, brownish, or sometimes yellowish. This is, however, a sure sign of its being weathered, to which it seems to be much disposed.

It is opaque, or, at the most, scarcely translucent on the edges.

The fresh specimens are of about the hardness of apatite; the weathered mineral is soft.

I found the specific gravity of a pure but not very fresh specimen to be 3.38. It usually is met with accompanied by triphylline, and both are frequently intermixed, and this compound mass, which is of a blackish grey colour, has an imperfect foliated cleavage; this, however, is not due to the substance under review, but is attributable to the triphylline.

Triphylline is met with in this substance sometimes only in patches, and is then of a blackish colour. Nodules sometimes are also found composed, externally, of triphylline, and of the mineral in question in the interior, and which, generally speaking, is more or less decomposed.

These nodules, upon which we may occasionally distinguish the planes of crystallization of triphylline, are almost always hollow, and the mineral in question is mostly botryoidal or kidney-shaped, and fibrous passing into the radiated structure; but, as we have stated, for the most part weathered, and very generally covered with a yellowish green coat.

Occasionally it is met with in small coarse patches in the substance of quartz or felspar, (Albite) and this variety is partly foliated with very fine scales, and sometimes perfectly compact. The fracture is uneven and without lustre.

This is all that I, at present, have to say of this mineral. Although the description we can give of it is not yet complete, there are sufficient grounds for our assuming it to form a distinct species, and for which, with reference to its colour in the mass and in the streak, I propose the name *Melanchlor*.

Probably it is allied to the mineral analysed by Karsten, and named by him *Grüneisenstein*, (green iron ore) and also to a phosphate of iron from the neighbourhood of Limoges, analysed by Vanquelin. I have not as yet, however, had an opportunity of examining these two minerals.

Experimental and Theoretical Researches in Electricity, Magnetism, &c. Fifth memoir. By WILLIAM STURGEON, Lecturer at the Honourable East India Company's Military Academy, Addiscombe; Superintendent of the Royal Victoria Gallery for the Encouragement of Practical Science, Manchester, &c., &c. (Continued from page 153.)

SECTION 2. *Chemical powers of Voltaic Batteries continued.—Proper Unit of Intensity for a maximum of the Decomposition of Acidulated Water.—Electro-calorific Phenomena.—Electro-magnetic Phenomena.—Electro-magnetic Telegraphs.*

279. I have already stated (268) that I had got 20 additional iron jars, to my former 10, and having also at command 50 pairs of Grove's battery, I proceeded to increase the series of each of these two kinds of battery, in order, if possible, to trace the decomposing action to the maximum series, or *unit of intensity*, of each kind, by which they may be employed to the greatest advantage in extensive arrangements, or when a great number of pairs are about to be used. In demonstrations before classes, and in lectures where the fact requires no further experimental illustration than by the usual way of exhibition, the decomposition of water by 10 pairs will be found amply sufficient, even in the most spacious lecture-room; for when we can command 25 cubic inches of the mixed gases per minute, a tolerably large receiver may be completely filled in a very short time: which forms a most surprising contrast to the puny tubesfull obtained by means of our earliest forms of battery. But, as on some occasions, we are desirous of showing the electro-decomposition of water to the greatest extent which our batteries are capable of performing, it will be interesting to know in what way to arrange them to the best advantage in the display of this phenomenon.

280. The two following tables will shew the results with various series from 10 to 20 pairs of each kind of battery:—

Table of Experiments on the Decomposition of Water, by various series of Voltaic Pairs, on the principle of Mr. Grove's Battery (253). The Electro-gasometer with large terminals (245) was the only one used in these experiments.

No. of Pairs in Series.	Cubic inches of Gas per minute.
10	14
11	13
12	12
13	12
14	11
15	11
16	10
17	10
18	10
19	10
20	10

281. The results in the above table show that a series of about 10 pairs is the proper unit for obtaining a maximum of decomposition of acidulated water; and that to employ either more or less would be attended with a considerable loss of action. The action generally is much less in this series of experiments than in those described in (); but that circumstance could have but little controul on the subject of the present enquiry.

282. *Table of Experiments on the Decomposition of Water, by various series of Voltaic Pairs, with the Cast-iron Battery (239). The large Electro-gasometer was used in these experiments.*

No. of Pairs in Series.	Cubic inches of Gas per minute.
10	24
11	27
12	31
13	28
14	23
15	22
16	22
17	18
18	17.5
19	17
20	16

283. The results of these experiments are highly valuable, both in a theoretical and a practical point of view, indicating, as they do, that a series of 12 pairs is, not only the proper unit for obtaining a maximum of decomposition, but that a more extensive series is absolutely detrimental, and will not give so much gas as by the employment of 12 pairs only.

Indeed, it is a most curious fact, that 20 pairs in series will only produce two-thirds of the decomposition that 10 pairs in series will do. From this fact it would appear probable that we might extend the series of pairs that would so far neutralise the decomposing action of the battery, as that it would have no power left to decompose acidulated water.

284. By combining 24 pairs of the iron battery into two series of 12 pairs each, in such a manner that both series should operate in concert on the acidulated water in the large decomposing apparatus, I was enabled to obtain an average of 60 cubic inches of the mixed gases per minute. The action of this battery, like that of all others, varies according to the condition of the zinc surfaces; being always the most powerful when those surfaces are smooth and well amalgamated. With new zincs, well amalgamated, I have obtained 64 cubic inches of the mixed gases per minute with 24 pairs in two series of 12 pairs each, operating in concert.

285. There is another circumstance yet to be noticed in the decomposition of acidulated water; and it is probable, I think, that the same circumstance may have an influence on other compounds. The large decomposing apparatus which I employed in these experiments has one of its platinum terminals about twice the size of the other; and when the larger one is connected with the positive pole of the battery, the decomposition of the water is carried on to a greater extent than when the connexions are made in the opposite way. In some cases, a difference of two cubic inches of gas per minute is obtained by the action of a battery of 10 pairs. Hence it will be necessary to mention, that in all the preceding experiments, the positive pole of each battery was connected with the larger platinum terminal, and, consequently, the negative pole with the smaller terminal. I am very far from supposing, that even these large terminals are sufficiently extensive for showing the maximum of decomposing power of any of the several batteries which I have employed. I think it probable that, by employing terminal surfaces, at least twice as extensive as those in the the larger decomposing apparatus, (245) a considerable increase of decomposition would be obtained; although for those batteries which do not afford much power, these terminals would be much too large. Hence it would be in vain to look for any one *pair* of terminal metals that would afford a maximum of decomposition for every kind of battery. And the fallacy of any indications of the powers of large batteries, which can be obtained by the decomposition of acidulated water from *small terminal wires*, is now too obvious to require any farther inquiries respecting it.

Moreover, since the decomposition of water is a phenomenon of one particular class only; and that other classes of phenomena, quite as important as the electro-chemical are displayed to the greatest advantage by very different arrangements of voltaic batteries to that which gives a maximum of chemical action, it would be absurd in the extreme to continue the term *Voltameter*, to any piece of apparatus which does not indicate the powers of voltaic batteries, in the production even of *one class* of phenomena; and which gives no idea whatever respecting the powers of batteries in the production of other classes. It is well known to the scientific world, that I have given to the water-decomposing apparatus, the name of *Electro-gasometer*; a term which cannot possibly lead any one into error, because it shows the absolute quantity of gas liberated by each battery employed, and presumes nothing more: and as I have shown that the quantity of gas liberated, by any *individual* battery will depend on the extent of surface which the platinum terminals exposes to the water in the instrument, it is obvious that no individual electro-gasometer can indicate the maxima of decompositions which different kinds of voltaic batteries are capable of displaying. The instrument which we continue to call a *galvanometer*, is in precisely the same predicament as the voltameter; because it indicates nothing more than electro-magnetic deflections. The proper name for this instrument would be the *electro-magnetometer*: but on this point I shall not dwell at present, as I shall shortly have an opportunity of interweaving its investigation into others which will form the subject of another memoir.

The Calorific Effects.

286. With respect to the calorific class of phenomena, I have not been able to prosecute my inquiries to a sufficient extent to ascertain the relative powers of the batteries already described. The experiments which I have hitherto made for this purpose have been with comparatively short conductors, such as are usually employed at the lecture table, or for illustrations in our large Exhibition-room. Through a circuit of 200 feet of copper wire of 1-12 diameter, a series of ten pairs of the iron battery ignites platinum for the explosions of gunpowder, as decidedly as by the employment of any other battery whatever; but I am not aware of the maximum distance from the battery at which gunpowder could be exploded by the calorific powers of ten pairs, but there can be no doubt of a series of ten pairs accomplishing the ignition of gunpowder at a much greater distance than 200 feet, if the copper conductors were sufficiently thick. I believe that Colonel Pasley employs copper conductors of half an inch diameter, which, perhaps, is not too much for facilitating the propagation of the electro-calorific powers through long conductors. By the employment of such capacious conductors, the iron battery will be found to answer all the purposes of springing mines, blasting rocks, &c., or in any other capacity in which gunpowder is to be exploded by voltaic electricity. The *quantity* of electricity excited by this battery is very great, and its propelling powers may be increased by the addition of a few pairs to the series.

287. In the Exhibition Gallery of this Institution we proceed through our daily illustrations of the decomposition of water,—the ignition of metals, and Colonel Pasley's operations against the wreck of the Royal George, with an iron battery of eight pairs only. For the ignition of short thick metallic wires, the iron battery is far superior to any of the others; when operating on long thin wires of platinum it is not so good as Grove's, though for the ignition of copper wires, which are better conductors, it is still superior. Hence there is an immense *quantity* of electricity yielded by the iron battery, but it is not of high intensity.

Description of a Series of the Iron Battery.

288. When an extensive series of the iron battery is employed, there is a considerable quantity of hydrogen gas liberated at the surface of the iron, which is an annoyance of which Daniell's and Smee's are free: but by arranging them under a rectangular cover or hood of japanned tin or zinc, as in fig. 2, plate vi., the hydrogen is prevented from making its escape into the room, and the operator experiences no inconvenience whatever. The hood rests on a stout board, round the upper side of which a deep groove is made for the reception of the lower edge. Near to the bottom of the hood, in one of its ends, is a small neck, out of which the common air makes its escape, being displaced by the hydrogen which collects and floats in the upper part. The groove on the upper side of the base-board, is intended to hold

water in case the battery should be kept in action for a long series of experiments, and the hood become filled with hydrogen. In such cases the neck near the bottom of the hood is to be corked up, and the bent tube at top, which passes into the water of a small tray, or pneumatic trough, is to be opened, and its orifice covered with a glass receiver in the usual way for collecting gases, as seen in the figure. When the receiver is full of hydrogen it may be removed from the trough, and the gas transferred to another vessel, and preserved for other purposes. Fig. 1, shows one of the iron jars with its zinc cylinder attached. Fig. 2, represents a series of eight pairs, covered with the hood, and connected with the electro-gasometer by means of two stout copper wires, which pass through the board, and rise sufficiently high above the upper surface to be united, within the hood, with the poles of the battery. The platinum terminals which ought to have appeared in the glass vessel, nearest to the battery, are, by mistake, wanting in the figure.

Our current expenditure, in this department, is considerably less by the employment of the iron battery than it otherwise would be if any other kind of battery were to be used for similar purposes.

Electro-Magnetic Phenomena.

289. The relative electro-magnetic powers of any two batteries of large dimensions and in vigorous action, are not easily ascertained on small masses of iron; because even the feebler battery may be of sufficient power to magnetize the iron to its maximum point. But by operating on large pieces of iron, or on moderate sized ones, with a single pair of voltaic metals of small dimensions, we have a pretty fair opportunity of arriving at decisive results.

The electro-magnet belonging to this Institution is made of a cylindrical bar of soft iron, bent into the form of a horse-shoe magnet, having the two branches parallel to each other, and at the distance of 4·5 inches. The diameter of the iron is 2·75 inches, it is 18 inches long when bent. It is surrounded by 14 coils of copper wire, 7 on each branch. The wire which constitutes the coils is 1-12th of an inch diameter, and in each coil there are about 70 feet of wire. They are united in the usual way with branch wires for the purpose of conducting the currents from the battery. The magnet was made by Mr. Nesbit. With this magnet and a battery of Professor Daniell's construction we have obtained the following results:—

Experiment.	Number of cells.	Weight sustained.	Weight which broke off keeper
1	19 in series	10 cwt.	10½ cwt.
2	19 do.	11½	12
3	1 do.	8½	9
4	10 do.	10	10½
5	10 do.	11½	12
6	16 in series of 4 each	12	12½
7	16 do.	12½	13

300. The greatest weight sustained by the magnet, in these experiments, is 12½ cwt., or 1386lbs., which was accomplished by 16 pairs of

plates in four groups of 4 pairs in series each. The lifting power by 19 pairs in series was considerably less than by 10 pairs in series; and but very little greater than that given by one cell, or one pair only. This is somewhat remarkable, and shows how easily we may be led to waste the magnetic powers of batteries by an injudicious arrangement of its elements.

301. Two experiments were then made with the electro-magnet (287) and a single pair of metals, excited by a strong solution of nitric acid. The metals were copper and rolled zinc, each formed into a cylindrical scroll and placed in a porcelain jar, 8 inches high, and 5 inches diameter.. The first experiment was made when the acid solution covered only one inch high of the lower edges of the metals: and the second experiment when the pot was filled to the brim.

	Weight sustained.	Weight which broke off keeper.
First experiment	8½ cwt.	9 cwt.
Second ditto	10 cwt.	10½ cwt.

In the first of these experiments the voltaic metals in action, exposed a surface of about 40 square inches to the acid solution, and the weight sustained was precisely the same as that sustained by using one pair of Daniell's form, which exposes a metallic surface of about 360 square inches (274.) Indeed, I have always found that the most vigorous magnetic action of any battery is excited by acid solutions: and if a pretty good share of sulphuric acid be not used in Daniell's battery, its action in the display of both chemical, magnetic, and calorific phenomena, is exceedingly low.

302. *Experiments made with the electro-magnet (287) and the cast iron battery (288)*

No. of pairs in series.	Weight sustained.	Weight which broke off the keeper.
8	12½ cwt.	13 cwt.
4	13 cwt.	13½ cwt.
Single pair	10½ cwt.	11 cwt.

By comparing the two sets of experiments, it will be found that the iron battery has a considerably greater magnetizing power than the Professor Daniell's form, both when in series, and in single pairs.

303. By employing iron for one of the voltaic metals, we have an opportunity of making a battery and an electro-magnet of the self same materials, or if you please, of converting the battery into a magnet. A battery-magnet of this kind, which I made some time ago, is represented by fig. 3, plate vi. It consists of two short pieces of musquet barrel, the lower end of each of which is plugged up by a solid piece of iron, and thus welded together to make the plug and barrel one piece of iron. The upper ends of the tubes are joined together by a cross piece of flat iron through which they pass, as seen in the figure: and the lower ends are filed flat and smooth for close adaptation to the keeper. Close to the lower end of each branch is soldered one end of a covered copper wire, about one-tenth of an inch in diameter, which is afterwards coiled round the barrel to the top, where it terminates in a cup for holding mercury. A narrow slip of amalgamated rolled zinc, with its connecting wire, is then placed in each barrel, and the

connexions made as seen in the figure. When charged with diluted sulphuric acid, this battery-magnet will carry about sixty pounds.

Electro-Magnetic Telegraphs.

304. The idea of forming telegraphic communications by electricity, seems to be of considerable date; for almost as soon as the fact became known, that conducting wires had the power of transmitting the electric influence instantaneously to distances of several miles, the idea occurred to several electricians that correspondence between distant parties might be accomplished by electric action. So long ago as the year 1748, Dr. Watson, the late Bishop of Llandaff, with several other philosophers, made experiments at Shooter's Hill, which showed that electrical discharges from a Leyden jar, could be propagated through a distance of upwards of four miles, without any appreciable loss of time, although a considerable portion of the circuit was formed of land and water. The success of these experiments, appears to have given rise to the first ideas of forming electric telegraphs, by means of which distant parties might hold correspondence, and mutually transmit their ideas, or communicate intelligence with the speed of lightning itself.

From the time that Dr. Watson made his experiments at Shooter's-hill there have been many and various contrivances for applying electric agency to telegraphic communication, some of which display great ingenuity and skill in managing so formidable an agent.

Winkler, at Leipsic, before 1750, discharged Leyden jars through very long circuits, in some of which the River Pleiss formed a part; and Le Monnier, at Paris, produced shocks through 12,789 feet of wire: and it is said that Bétancourt, at Madrid, discharged electric jars through a distance of 26 miles. Perhaps the most decisive attempt hitherto made to establish electric telegraphs, was by Mr. Ronalds, at Hammersmith, about the year 1816.

305. The discovery of the voltaic battery affording new resources, and novel means of communicating intelligence to distant stations, its powers soon became available in this capacity, and was brought into some degree of repute by the ingenious application of them by M. Sömmering, in the construction of his galvanic telegraph.*

306. The discovery of electro-magnetism by Prof. Ørsted, of Copenhagen, opened an entirely new field for telegraphic speculation: and I believe that M. Ampere, in France, and Professor Barlow, in this country, were about the first philosophers who attempted electro-magnetic telegraphs. Since that time Professors Morse,† Ganss, Weber, Steinheil,‡ and Wheatstone, and Mr. Davy, late of the Strand, London, have been inventors of electro-magnetic telegraphs; each

* Sömmering's telegraph operated by the decomposition of water from a series of gold pins, any of which he could bring into play whenever he pleased by touching a key, which brought into the circuit a wire properly connected with the pin from which decomposition was intended to proceed.

† See *Annals of Electricity*, Vol. 2, p. 116.

‡ *Ibid.* Vol. 3, p.

instrument displaying some peculiarity which its author considers an advantage over the rest.

307. In describing a new electro-magnetic telegraph, I am necessarily impelled by a similar feeling to that which urged my predecessors to bring their respective inventions before the public; and I cannot resist the idea that there will be found a peculiar simplicity both in the structure and management of the telegraph I am about to describe. Indeed I shall point out the structure of two distinct telegraphs, having the sign common to both. Also a third, differing very materially from the other two.

308. In one of these telegraphs I use six soft iron bars, bent into the form of horse-shoe magnets, and covered with copper wire spirals, in the usual way, for converting them into occasional magnets by electric currents. To each magnet is a short bar of soft iron for a keeper or cross piece, which is attached to the shorter arm of a lever, of the first order; and to the extremity of the longer arm of the lever is attached a circular card. The arrangement of one of these pieces of apparatus is shown by figs. 6 and 7, plate vi., the former being a side view, and the latter an end view of it: *m*, in both figures, represents the magnet, *i* the cross piece, *a b* the lever, and *f* the fulcrum. The cards at the longer extremities of the six levers are numbered 1, 2, 3, 4, 5, 6, which, individually, and by a series of simple combinations, form all the signals that are required.

309. When the levers are in the position shown in fig. 6 and 7, the magnet is out of action, in consequence of the battery circuit being interrupted. If, now, the battery circuit were to be closed, the magnet *m* would immediately be brought into action, and its attractive force would bring down the cross-piece *i*; which, being attached to the shorter arm of the lever, would raise the longer arm with its card and sign, into the position of the upper dotted circle, where it becomes visible through a circular opening in the face of the instrument, as at (5), in fig. 5. When that particular sign has appeared the required time to be observed, the battery circuit is opened, the magnet *m* loses its power, and the longer arm of the lever preponderating, again falls down to its first position, and the card with its sign disappears.

310. The face or dial of this telegraph is represented by fig. 5, which may be either of painted wood, or metal, silvered in the manner of clock faces, or barometer scales. On the upper part of the dial there are six circular openings for the occasional appearance of the cards, with their figures, which are attached to the longer arms of the six levers (see fig. 6.) These levers with their magnets, &c, figs. 6 and 7, are placed behind the dial in a suitable case, and in such a manner that the figures on the cards may appear at the circular openings whenever their levers move upwards by the attractions of their respective magnets at the other, or shorter arms: and to disappear below those circular openings, when the magnets are out of action. To accomplish this

* Below the circular openings in the dials plate there are arranged the signals which are to represent all the alphabetical letters that are necessary for the spelling of words. The signals are thus continually before the eyes of the operator, and are too simple to miss being understood.

latter effect, the face of the cross-piece of iron, which is attached to the short arm of each lever, must be covered by a card, or a film of some non-ferruginous matter, which will prevent close contact of the iron and magnet. By this arrangement of the apparatus it is a matter of no consequence in what way the magnetic poles are arranged, because the attraction of the cross-pieces, attached to the shorter arms of the levers, will take place as well with one arrangement as with another. But for uniformity, we will suppose that the coils on the magnets are all of the same kind, and that the north poles are to be in one and the same direction, towards the left hand for instance, to a person facing them, then those extremities of all the coil wires which were situated in one direction, might be collected together in one bundle, and either continued to the station where the battery is situated, or soldered to one stout copper conductor, at some short distance from the magnets, which conductor would become a general *fixed channel* between all the magnets at this station, and the battery at the other station. The other six ends of coil wires must be insulated by silk covering, and continued to the battery without metallic contact with each other. At the battery station these six insulated wires are to be attached to six wooden or ivory keys with springs, like the keys and springs of a piano-forte: by the downwards motion of which, the extremities of the wires become immersed in a long trough of mercury, connected with the opposite pole of the battery to that which the other conductor is attached to. On the top of each key is to be a conspicuous figure, corresponding to the figure which is to appear in the dial plate at the other station, so that when one finger is placed on key 2, and another finger on key 5, the magnets 2 and 5 at the other station are brought into play, and by attracting their respective pieces of iron, the figures 2 and 5 make their appearance on the dial as seen in figure 5, plate vi, and the letter p is understood. By these means, twenty-one of the letters of the alphabet can easily be represented without a possibility of error, either in the manipulation at the one station, or in the reading at the other; unless, indeed, there be a deficiency of attention which would incapacitate the attendants for employment at any telegraph whatever.

311. The keys of this telegraph are sufficiently near to each other to permit the fingers to press on any number of them at one time, and, if necessary, the whole of the magnets may be brought into play at once, by the application of three fingers of each hand to the keys. By these means the numerals may be grouped into combinations of three, four, five and six, and thus, without the slightest confusion, a considerable number of signals would be obtained, which might represent words, or whole sentences, which would greatly expedite the transmission of intelligence from one end of the line to the other.

312. There is a very great advantage by employing the numerals for signals. Not only because they are not so liable to lead to confusion as by the employment of the alphabetical letters, when used in combinations or groups; but because the subject of communication may be kept a perfect secret from one end of the line to the other; which is a most essential consideration in government expresses, and very often in those of mercantile affairs also.

In this telegraph a seventh magnet is employed to ring a warning bell, as first proposed by Professor Steinheit.

313. Although in the telegraph already described I employ soft iron magnets and levers to bring the signals into view, I am of opinion that magnetic needles in coiled conductors, or electro-magnetic multipliers, will be somewhat more prompt in their motions than the lever, at great distances from the battery. I therefore propose to make the necessary signals by means of magnetic needles, which can be moved with the same arrangement of conductors as that already described. And although I have only used six numerals for the signals, I am very far from supposing that the working of an electro-magnetic telegraph is facilitated or simplified by using a small number of original signals, or by having a small number of conductors. The simplest method of *spelling* words would be to have a needle for each letter of the alphabet, and the telegraph could be *made and worked* as easily by 24 needles as by a smaller number. And the words and sentences, which could be signified by combining them in pairs, or in groups of two each, would afford great facilities for the rapid transmission of ideas from one end of the line to the other. The needles could be placed in three horizontal rows, one above another, on a vertical dial plate.

314. I have shown a dial plate, in fig. 8, plate vi., on which are placed 10 needles with their respective figures or signs. As the needles can be deflected in only one direction, viz., with the north end towards the figure which belongs to it, there can be no mistake in understanding what sign is to be understood. I believe that any of these telegraphs will be found much simpler than those already before the public. They are capable of producing many more signs than any other known, and may be made at a less expense.

WILLIAM STURGEON.

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encouragement of Practical
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XXXIX.—On the action of Metallic Tin on solutions of Muriate of Tin. By AUGUSTUS A. HAYES.

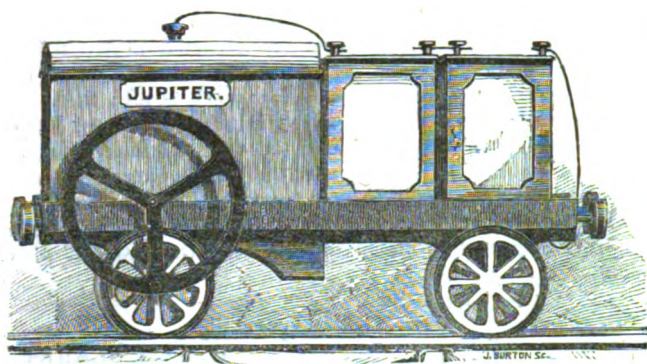
It has been long known to those who frequently dissolve tin in muriatic acid, that under some circumstances, the metal after it has been dissolved is precipitated. It sometimes presents large sections of octahedral crystals, at others, long prismatic needles, which are so arranged as to form skeletons of such sections. In this Journal, Vol. xxvii, p. 255, Mr. W. W. Mather has described some experiments having a similar result. The interest which has been excited of late by notices of the non-action of metals in acid solutions and in relation to chemical action of a similar kind, has induced me to publish the facts which I sometime since observed.

When tin is dissolved in muriatic acid, either by gradual action under exposure to air, or by the aid of heat, a solution containing

an excess of acid is obtained. This solution may be concentrated to a sp. gr.=1.750, and retains its fluid form at or above 60° F. Although an excess of tin is present, the solution thus obtained is always acid. After decanting the clear solution, the tin used in excess with its impurities remains. Generally, after a few days exposure, the matters left in the solution vessel change in appearance. The dull, corroded fragments of metal become frosted over, with bright needles of tin, and beautiful arborescent forms are seen. On studying the circumstances, I have found that the effect is due to electrical action. One portion of the undissolved tin, becoming a *positive* electrode, while another portion of the same mass assumes the state of a *negative* electrode, and precipitation of the dissolved tin takes place on it. Numerous cases of like action are known to chemists, where a part of a bar becomes indifferent to a concentrated solution, although a positive state is exhibited at another part, and active solution of the metal is taking place.

For the purposes of experiment, a solution of muriate of tin, of sp. gr. about 1.650, contained in a cylindrical vessel, may be carefully covered by half its volume of an acid solution of the same, having a sp. gr. about 1.20. The two fluids should not be mixed more than the slight diffusion which will take place. After placing a flat bar or plate in an inclined position, so that it passes through both solutions, the effects become immediately perceptible. That part of the bar which is within the diluted solution takes the *positive* state. A few minute bubbles of hydrogen form and escape, if the solution is quite acid. Precipitation of metallic tin commences near the line of contact of the two solutions, and extends throughout that part of the bar immersed in the denser solution. If the diluted solution is not rendered acid by the addition of acid, hydrogen is not perceived, and the action is more gradual. In either case the precipitation continues until the two fluids have attained the same electrical relation to the bar. If after the precipitation has ceased, water be carefully poured upon the surface of the fluid, it will form a stratum of very dilute solution. That part of the bar not before immersed takes the negative relation to this solution, and the same kind of precipitation follows as had taken place in the lower solution. The positive part of the bar retains its state unaltered under the new conditions, and the line of separation is as clearly defined as in the first case. If a solution mixed with crystals be used, instead of a moderately concentrated solution, they are not decomposed under the above conditions. The presence of atmospheric oxygen has been supposed to influence this action. Such is not a correct statement; by exposure to atmospheric vapour, strong solutions of muriate of tin become weaker, and any masses of undissolved tin, projecting into the weaker solution, will decompose the denser solution below. In numerous trials, I have found all the cases of precipitation referable to different states of two solutions resting in contact.

Roxbury Laboratory, March 16, 1840.

XL.—Description of an *Electro-Magnetic Locomotive Carriage.*

By Mr. URIAH CLARKE.

Leicester, Sept. 15, 1840.

MR. STURGEON,

Sir,—I take the liberty of forwarding to you a wood engraving of my electro-magnetic carriage, which has been working at intervals for the last two months on a circular railway at the Leicester Exhibition. This carriage when the battery is charged weighs 60lbs., and will run at a considerable speed for two hours and a half. I have lately increased its power so much that it has drawn a train of carriages weighing, together with itself, 112lbs. The battery by which this effect is produced consists of merely two pair of plates on Professor DANIELL's sustaining principle; each pair of plates is charged with about one pint and a half of solution. The carriage is propelled by an arrangement of machinery on the reciprocating principle, which principle I claim to be the first inventor of, as far as regards the production of an efficient motive power. The engraving represents an exterior view of the carriage as it appears upon the railway. I do not describe the interior construction as I have adopted a particular mode of arrangement in the carriage which I have not yet made public. About six months since, you will remember, I communicated to you a drawing and description of an engine which I invented and constructed more than two years ago, on the reciprocating principle: that communication (embodying the essential features of my invention) I find inserted in the July No. of "The Annals," and in the Aug. No. I observe a description of a projected new engine by Mr. Thos. Wright, in which he appears to have copied my engine to a certain extent, adapting to it a new mode of arrangement, which he no doubt considers to be an improvement, but from actual experiments which I have made (for I have constructed several engines on this

reciprocating principle, and have adopted a variety of modes of arrangement since I contrived the one which I communicated to you) I am led to think that an engine constructed on Mr. Wright's mode of arrangement will be very feeble in its action: it is more than doubtful to me whether it will ever work at all; the loading of the reciprocating bar, I think, seems rather injudicious, but this is not the most objectionable part of the alteration which he suggests. I would also observe, that the lengthening of the stroke of the lever by merely removing the fulcrum is by no means a new idea. His manner of making the magnets is, I believe, original, and in this respect shall be anxious to hear the result of his experiment.

I am, Sir,

Yours respectfully,

URIAH CLARKE.

MISCELLANEOUS ARTICLES.

XLI.—BRITISH ASSOCIATION PROCEEDINGS, THURSDAY, SEPTEMBER 21, 1840.

SECTION B.—*Chemistry and Mineralogy.*

In this Section, the President, Dr. T. Thomson, F.R.S., took the chair. The first paper read was, "On the peculiar odour evolved in certain electro-chemical decompositions," by Professor Schöbein. In the absence of the author, Mr. E. Solly read the paper to the Section.

M. Schönbein has undertaken a series of experiments, in order to ascertain the circumstances under which the odour is evolved in electro-chemical decompositions; the causes which influence its production; and, if possible, the principle to which its appearance is to be attributed. After describing the period at which the odour is produced, he goes on to observe, that the odour is evolved on the decomposition of water, dilute sulphuric acid, and many oxysalis; dilute sulphuric acid yielding it in largest quantities. The author found, on collecting the oxygen gas evolved at the anode from a solution capable of evolving this odour, that it might be preserved for some time by inclosing the gas in well stopped bottles. From the characters possessed by this oxygen, he was led to consider the odour due to the presence of a minute portion of a new and hitherto wholly unknown substance, of considerable importance in many natural phenomena; and he has therefore named it, from

its most evident character, ozone. Its properties are as follows:— It is only evolved from solutions containing it, by perfectly clear electrodes of platinum of gold, while charcoal and the more oxidisable metals are unable to cause its appearance. It can only be obtained from a cold solution. When a piece of one of the oxidisable metals, such as lime, iron, &c., is placed in a portion of oxygen impregnated with ozone, that peculiar substance is almost immediately absorbed, and the oxygen becomes inodorous. When perfectly clear and dry plates of gold are immersed in oxygen containing ozone, they acquire a negatively electric state of polarity. The plates thus polarised continue their electric powers in air for a considerable time, but rapidly leave it when plunged in hydrogen gas, in which, if retained a sufficient time, they acquire an opposite state, becoming positively polarised. After comparing these effects with those produced by the odour peculiar to common electric sparks and brushes, he states that both from its electro-motive power, and likewise from its strong affinity to metals, it is evidently similar to chlorine, bromine, and iodine. Its non-appearance, when water is decomposed by electrodes of the more oxidisable metals, he attributes to its entering immediately into combination with the metals; and he considers that, when the solution is heated, the affinity of the ozone for metals is so much increased, that it is even able to combine with gold and platinum; thus accounting for its disappearance when heated. By this theory, all the phenomena attendant on its evolution may be easily explained; and it hence becomes very interesting to search for traces of this widely-diffused substance. M. Schönbein considers the smell perceived whenever bodies are struck by lightning is probably due to a small portion of ozone being set free, and relates a case of a church lately struck by lightning, which fell within his own observation, in which the surrounding buildings to a considerable distance were filled with a bluish vapour and peculiar pungent odour.

The second paper read was by Mr. E. Solly, "On the best method of Bleaching Vegetable Wax." Mr. Solly, after referring to a number of experiments which he had made during the course of the summer, to discolourise vegetable wax, stated he found the following to answer the purpose most completely, by which the wax was bleached in a few minutes, and greater effect of discolouration was produced than by the mere passage of chlorine for half an hour. This method consisted of bleaching by pure nitric acid, by melting the wax, pouring in a small quantity of sulphuric acid, composed of one part of oil of vitriol to two of water, and then stirring in a few crystals of nitrate of soda, the whole to be agitated with a wooden stirrer and kept heated. Nitric acid is then evolved in considerable quantity and purity from a large surface, and in such a manner that all the acid evolved must necessarily pass through the melted wax. This method answers the purpose very completely, the process is cheap and rapid, and the residuum, being merely a little solution of sulphate of soda, is very easily removed.

The Chairman remarked that this was a very simple mode of

bleaching wax, and a general knowledge of it might be extremely useful.

Professor Gregory read a communication "On the pre-existence of Urea in Uric Acid." By the action of peroxide of lead on uric acid, Liebig and Wöhler obtained from it oxalic acid, allantoin, and urea, and they considered the latter as existing in the uric acid, combined with urile. The author having found that urea, unlike most organic substances, resists the oxidising agency of permanganate of potash, thought, if urea could be obtained from uric acid by the action of that salt, the argument for its pre-existence would be much strengthened, as, if only the elements of urea were present, the oxidising agency of the permanganate would most likely prevent its formation. On trying the experiment, a large quantity of urea was obtained along with oxalic acid, and a new acid, probably formed by the oxidation of allantoin. The author farther described the acetate of urea—a salt which was formed in the experiment. Professor Gregory then exhibited a new process, communicated by Professor Liebig, for preparing the new, singular, and beautiful compound, termed murexide by Liebig and Wohler, and purpurate of ammonia by Prout. This process is quite certain, and very productive. It consists in adding a boiling solution of seven grains of alloxan, and four grains of alloxantine, in 240 grains of water, to eighty of a cold and strong solution of carbonate of ammonia. The mixture instantly acquires a deep purple colour, and on cooling deposits the golden green crystals of murexide.

Professor Graham asked if Dr. Gregory thought this a good process for procuring murexide, as it could be purchased in London from 4s. to 5s. per lb. The question led to no farther remarks.

Dr. Schafheutl, of Munich, read a very valuable paper on the relation of forms to chemical composition, in which he showed that the varieties of graphite, which are frequently met with, depend on difference of chemical composition. He also described his method of analysing graphite or plumbago, by means of sulphuric and nitric acids.

The last paper read was by Dr. Schafheutl, on a new compound of arsenious acid and sulphuric acid, which appears to be the destructive agent to vegetable and animal life, in the vapour emitted from the copper works at Swansea in South Wales. The view elicited by this interesting discovery of Dr. Schafheutl is likely to throw much light on some peculiarities observed in poisoning by arsenic.

FRIDAY:

DR. T. THOMSON IN THE CHAIR.

Before the regular business commenced the Muraxide produced by Dr. Gregory in his experiment yesterday was handed round the room for the examination of the section.

Professor Graham was then requested to take the chair, while the president read a paper "On the most important chemical manufactures carried on in Glasgow and the neighbourhood." The

manufacture of iron, sulphuric acid, bleaching powder, or chloride of lime, alum made at Hurlet and Campsie, precipitate of potash, acromate of potash, tartaric acid, ascetic acid, pyroxylis spirit, iodine, soap, bleaching of cotton cloth, Turkey red dyeing, glass making; cudbear and gas were enumerated, and the manufacture of them fully explained.

Professor Graham remarked that the section were greatly indebted to Dr. Thomson for collecting such a mass of valuable information on the manufactures of Glasgow and neighbourhood.

The second paper read was by Mr. Connell, "On the Voltaic decomposition of Alcohol." The author endeavoured to show that, by dissolving a small quantity of potassium in pure alcohol, and then subjecting the compound to voltaic action, water was obtained.

Dr. L. Playfair read the next paper, which was by D. R. W. Glover, "On a new process for obtaining hydrobromic acid, and hydriodic acid." The author proposed the employment of bromite and iodite of bromine as a very convenient source of the above named hydrobromic, in atomic proportions.

Professor Bunsem read the next paper, which was on the compounds of a new radical compound, called "Kakodyle." The process by which this compound is obtained is exceedingly dangerous, and the author, in his experiments, has been several times severely injured. Arsenic is a principal ingredient in the compound.

The next paper read was by Dr. Mohr, on a new mode of preparing Morphia. The principle of the new method of preparing morphia consists in dissolving the morphium in caustic lime by means of heat, and precipitating the filtered liquor by muriate of ammonia. The lime is neutralised by the muriatic acid of salt, ammonia set free, and the morphia precipitated. In this process the morphia is obtained in a crystalline and very pure state, without the alcohol. This mode of operating is as follows:—The opium is dissolved in boiling water and strained, this operation repeated twice, the liquors concentrated by evaporation, boiled with caustic lime, strained again, and mixed while hot with powder of sal ammoniac.

Dr. Gregory said he had had a great deal of experience in preparing morphia, and he was quite satisfied that Dr. Mohr's was the best, both for preparing small quantities, and for class experiments. He was sure it would be universally adopted as soon as known.

Dr. R. D. Thomson read a paper by Mr. Sturgeon—"On a peculiar class of Voltaic phenomena." The section then adjourned.

XLII.—Organic Chemistry.—*M. Majendie communicated the following Letter, which was addressed to him by M. Donné.*

SIR,—Permit me to mention to you some new results of my researches on urine, for the assistance of the interesting observation you made some time ago, on the production of oxalate of lime, determined by the use of sorrel.

From the commencement of the spring, in the urines I have subjected to microscopic analysis, very beautiful and numerous crystals, of a cubical form, and bearing a great analogy at first sight to the crystals of marine salt.

But on the one hand chloride of sodium is too soluble to be deposited in urine without previous evaporation; on the other hand, the crystals of which I speak are insoluble in cold or even hot water: besides, in rolling them on a plate of glass, we soon perceive that instead of being cubes, they are formed of two pyramids of four faces; generally connected by their bases, which gives the crystal the appearance sometimes of a cube, and sometimes of a lozenge, according to the position it takes.

These crystals are insoluble in acetic and soluble in nitric acid without effervescence; collected and well washed, calcined and burnt on a plate of platina by means of a pipe, they leave a white matter, which placed with a little water on red turnsol paper, instantaneously changes to blue. Hence this matter is evidently lime proceeding from the decomposition of the oxalate of the base. And in fact, it is only necessary to eat a certain quantity of sorrel to produce in the urine an immense quantity of these crystals: in less than two hours after eating it, the fluid deposits thousands of them by cooling and being undisturbed.

Thinking this observation would interest you I hastened to communicate it. I will add, with regard to nitric acid, that independently of the causes you have mentioned as its production, I am assured, by the comparative observation of the alimentary regimen and the composition of urine, that stimulants of the nervous system, such as coffee, tea, and even smoking tobacco, determine infallibly the formation of a great quantity of uric acid, which crystalise in yellow rhomboidal spangles by cooling.

Hence we may conclude, as you have done, the precautions proper to be taken in case of a tendency to this sort of gravel, and that, also, we might take account of the presence in excess of uric acid in the symptom of diseases with regard to the state of the nervous system.

XLIII.—*Note on a new arrangement of Steam Guages intended for high-pressure steam boilers.* By M. E. PECLET.*

High-pressure boiler guages are always of compressed air; for inverted syphons, open and filled with mercury, would become too hot. But the common guages are always inexact, at least I have not met with one, which after a few months use, has not experienced a considerable diminution in the air which it at first contained. This diminution in the volume of air sometimes results from a sudden abatement of pressure in the boiler occasioned by a too rapid injection of cold water, and in all cases the slow action of the air on the mercury, which progressively diminishes its volume. Besides which the constructors do not correct the graduation of the scale for the variation of the level of the mercury.

The following arrangement completely obviates all these inconveniences:—

A glass ball, 3 or 4 centimetres in diameter, is mounted on a tube intended to communicate with the boiler, it is soldered by its lower part to a capillary tube, which descending vertically 8 or 10 centimetres, is bent and raised vertically as high as the middle of the ball, and is prolonged horizontally for about 50 or 60 centimetres; this capillary tube is open at its extremity, near which it has a thread, also the part of the capillary tube which is raised vertically is furnished with a small ball. We commence by dividing the horizontal part of the tube into portions of equal capacity, which presents no difficulty, the tube being open at both ends; after this mercury is poured in the large ball as high as the horizontal tube, and the apparatus is slightly inclined, so that the mercury may entirely fill the tube: the extremity of the tube is then inserted into a cork which closes a tube full of chloride of calcium, communicating by the other extremity with a vessel of hydrogen or carbonic acid; the apparatus is then inclined in the opposite direction, so that the mercury may retire as far as the origin of the divisions, and finally, the glass is fixed in the middle of the thread by means of the flame of a blow-pipe.

In an apparatus of this kind there are no corrections to be made for the variations of the level of the mercury, since it is constant: there is no fear of a loss of gas by a sudden diminution of pressure, on account of the ball placed in the ascending part of the capillary tube, which the expanded gas would fill previous to escaping: finally, the gas having no action on the mercury, the instrument will not become defective from time.

* Translated by J. H. Lang.

August, 1840.

XLIV.—*Analysis of a Chromic Iron Ore, first observed by R. C. TAYLOR, Esq., at Mahobal, near Gibara, Island of Cuba; by JAMES C. BOOTH, and M. CAREY LEA.*

1. *Description.*—This mineral has a black color, and shining metallic lustre, closely resembling Franklinite from New Jersey. It is moderately brittle, exhibiting a chocolate brown streak, when reduced to the finest powder. The mass consists of coarsely crystalline particles, aggregated together, with intervening talcose matter, of a lighter color and softer texture than the chromic iron. This crystalline structure is so evident, that triangular faces of the octahedron are observable in a majority of the specimens.

2. Before the blowpipe, it dissolves in a bead of borax or microcosmic salt, exhibiting the characteristic reaction of oxide of chrome.

3. *Analysis.*—To obtain a proper specimen of the mineral for analysis, it was coarsely broken up and separated from the gangue as far as practicable. It was then finely pulverized, and one gramme of it ignited with carbonate of soda and caustic potassa, in order to convert the oxide of chrome into chromate of potassa.

4. The fused mass was digested with water and thrown upon a filter, which separated the oxide of iron and that portion of the mineral which had not been decomposed, from the other constituent which passed through in solution. The filter was then treated with hydrochloric acid, which dissolved the iron, leaving the undecomposed ore on the filter. This was found to amount to .353.

5. The solution of the chloride of iron which passed through, was then digested with nitric acid, and the peroxide precipitated by ammonia. This amounted to .172. In a previous experiment, it was found to contain neither alumina nor magnesia.

6. The solution obtained by the first filtration (4), was next neutralized by nitric acid, enough being added to precipitate and redissolve the alumina. The latter was then precipitated by bicarbonate of soda and its weight found to be .1414.

7. The remaining solution was now evaporated to dryness with carbonate of soda, and treated with water. The magnesia thus rendered insoluble, was separated and amounted to .090.

8. In the solution from (7), there still remained the oxide of chrome, which was estimated by concentrating the liquid by evaporation and adding to it while boiling, hydrochloric acid and alcohol. The chromic acid, thus converted into oxide of chrome, was precipitated by ammonia and separated on a filter. The solution passing through, still contained a small portion of oxide of chrome and was therefore evaporated to dryness and digested with water. The oxide of chrome thus rendered insoluble, was added to that before obtained, and the weight of the whole amounted to .244.

9. *Conclusions.*—The streak of the mineral being chocolate brown, it is difficult to say whether this color arises from the protoxide of iron or the brown oxide of chrome, a problem of exceedingly difficult solution by chemical analysis. Supposing the iron, however, to be in the state of protoxide, the .172 will be reduced to .1544 of protoxide. Now if ignition with carbonate of soda and caustic potassa, left a portion of the mineral undecomposed, it may without great error be assumed that the iron in this portion has not been peroxidized by that operation, and that therefore .353 is the correct weight of the undecomposed portion of the mineral. By adding the several weights obtained, we have,

Oxide of chrome,	-	-	-	-	.2440
Protoxide of iron,	-	-	-	-	.1544
Alumina,	-	-	-	-	.1414
Magnesia,	-	-	-	-	.0900
Undecomposed ore,	-	-	-	-	.3530

.9828

This shows a loss of 1.72 per cent., which may be ascribed in part to errors in analysis, and partly, without impropriety, to a partial peroxidation either of the iron or chrome.

By omitting the undecomposed matter, and calculating the percentage of each ingredient, we find the mineral to consist of

Oxide of chrome,	-	-	-	-	38.742
Protoxide of iron,	-	-	-	-	24.516
Alumina,	-	-	-	-	22.452
Magnesia,	-	-	-	-	14.290

100.000

This result indicates that a portion of the talcose matter was included in the specimen, notwithstanding the care exercised in its separation. Viewing the alumina, with a little silica included in it and the magnesia, as belonging to the talc, we find the formula for the oxides of chrome and iron, to be 2 : 3, or $3(\text{FeO}) + 2(\text{Cr}^2\text{O}^3)$. The formula generally received for the pure mineral is $2\text{Cr} + \text{Fe}$, and leads to the supposition, that in the present case, a portion of the iron exists as peroxide, a view which is strengthened by the brown streak of the mineral.

Philadelphia, Dec. 5, 1839.

XLV.—Description and Analysis of a Meteoric mass, found in Tennessee, composed of Metallic Iron, Graphite, Hydroxide of Iron and Pyrites ; by G. TROOST, M. D., Prof. of Chemistry, Mineralogy and Geology, in the University of Nashville, Tenn.

During my excursions through East Tennessee, I had seen small fragments of native iron, and had heard of large masses of it, which were believed to be silver. It being considered a precious metal, all that was known about it, and the place where it was found, were kept a profound secret. Some less prejudiced inhabitant at last became acquainted with the nature of the metal, and its real value was made known. To the politeness of Col. Micajah C. Rodgers, of Serierille, I am indebted for a considerable quantity of it ; and the Hon. Judge Jacob Peck of Jefferson County, has also presented me with some small fragments. I am thus enabled to lay a description of this singular substance before the scientific public.

Having ascertained, as appears from the analysis below given, that this iron contains nickel, the mass must be considered of meteoric origin ; but it differs from most of the masses of meteoric iron hitherto described. The original weight of it is said to have been about 2000 pounds. The portions that I have seen, (as well as those which are in my possession,) present a singular heterogeneous mixture of metallic iron, carburet of iron or graphite, sulphuret of iron, (pyrites,) and hydroxide of iron, the latter, brown and yellow ; in some parts all four ingredients form a kind of homogeneous mixture.

The most abundant constituent, however, is the nickeliferous iron, and it composes about 95-100ths of the whole mass. It has partly a crystalline structure, and is in part, composed of grains or globules of various sizes and forms, merely agglutinated together, or sometimes separated by a thin flexible highly polished pellicle of graphite. The crystalline part is composed of laminæ of various thickness, in the form of equilateral triangles, which are separated from each other by very thin flexible pellicles, as mentioned above respecting the grains.

I expected to find these triangular laminæ placed in such position as to form octahedrons, or showing a cleavage parallel to the sides of a regular octahedron ; but this is not the case, as the cleavage gives a regular tetrahedron. I have one of these forms, which is about an inch from base to apex.

The metallic iron is also dispersed in small irregular-shaped masses through a hard, compact, brown hydrated oxide of iron. Throughout this the iron is also dispersed in invisible grains, to be detected only by the magnet, which attracts them when the substance has been reduced to powder.

This iron is malleable. I have in my possession a horse-shoe nail, which was made of it without having undergone a previous

preparation, but it is harder and whiter than common wrought iron. This hardness and color may be owing to a small quantity of carbon which it contains, or perhaps to the nickel; in its natural state, however, the color of the iron differs much in different parts. In some it is black, and has no metallic lustre; in others, it has a brilliant metallic lustre, and is then always much whiter than steel or common iron. It is then but little susceptible of being tarnished when exposed to the action of the air; the black part being merely tarnished, may be rendered white by a file; in some places it is covered with a kind of black varnish.

The substance which constitutes the greatest part of the remainder of the mass, is graphite. This substance is not easily distinguished from the common graphite or plumbago, except that it is a little harder than the common granular and compact varieties, and is also rather blacker, and makes a finer, blacker, and more distinct line upon paper than common plumbago. When rubbed with a hard body it assumes a bright metallic lustre. It is not pure graphite, but rather a mixture of graphite and metallic iron. The iron can be partly removed by a magnet when the graphite is reduced to powder, but a considerable portion remains mixed with the graphite, which, when acted upon with hydrochloric acid, is dissolved with a brisk effervescence of hydrogen gas.

The sulphuret of iron, or pyrites, occupies the smallest portion of the mass. This pyrites is not attracted by the magnet, nor does it seem to act upon the magnetic needle. It can easily be cut with a knife, and is consequently softer than common pyrites. It does not give sparks when struck with steel, another property which distinguishes it from common pyrites. It is easily soluble in diluted hydrochloric acid, with a brisk evolution of sulphuretted hydrogen gas, leaving a mixed powder of white and black in the fluid. It has a more or less sub-lamellar structure, in which no regularity can be perceived, and a color between bronze yellow and copper red, often tarnished.

The hydroxide of iron, which forms part of this mass, is a heterogeneous mixture of the varieties of the ore generally known under the names of brown iron ore and yellow ochre, and resembles this terrestrial mineral. Its color is generally brownish black, passing into liver brown. The external surface of the mass is covered here and there with the yellow earthy variety (yellow ochre); how far this covering extended, I am not able to say, as the mass was too roughly handled before any part of it came into my possession. Its fracture resembles that of the common compact brown iron ore. The blackish brown variety is so very hard, that the best file is immediately dulled upon it, and leaves particles of the steel on the surface of the ore. Nevertheless, the whole is not of uniform hardness; a part, particularly the liver brown, being scratched by the file.

Some small cavities in it are lined with lamellar crystals, resembling those of white pyrites.

This hydroxide, which serves as a matrix of the metallic iron, is

not, judging from my specimens, abundant in the interior of the mass, but the exterior of the mass is entirely made up of it. At some places it is about one inch thick, while at others it is no more than one quarter of an inch, showing here and there small points of the metallic iron piercing through it.

Such are the characters and appearances of this mass, of the date and circumstances of whose fall, nothing is known. It was accidentally discovered near Cosby's creek, in the southwestern part of Cocke County, East Tennessee, and as I mentioned above, was considered a silver ore. Indeed, there is yet a fragment of it in the hands of an inhabitant, who asks for it 1500 dollars—a sum, which would be some hundred dollars too much, if it were pure silver.

Chemical constituents of the different parts.

1. *Metallic Iron*.—100 grains of the metallic iron were dissolved in diluted hydrochloric acid, leaving a residue of half a grain of a black powder, similar to that obtained from the graphite. This solution being treated with nitric acid, to convert the protoxide into peroxide, was precipitated by pure ammonia. The precipitate being washed and ignited, gave 124 grains of peroxide, = 87 grains of iron. The ammoniacal solution gave 16 grains of protoxide of nickel, = 12 grains of metallic nickel, with a trace of cobalt; loss, half a grain.

Iron,	87.0
Nickel,	12.0
Carbon,	0.5
Loss,	0.5

100.0

2. *Graphite*.—50 grains of the graphite being pulverized and freed by a magnet from intermixed iron, were acted upon with diluted hydrochloric acid. An effervescence took place, with expulsion of hydrogen gas, owing to metallic iron, which was so intimately mixed with the graphite, that it was not attracted by the magnet. After the effervescence ceased, it was heated in order to dissolve every thing that was soluble. The insoluble part was washed and dried; it was pure carbon, and weighed 46½ grains.

The hydrochloric solution being treated with nitric acid, to convert the protoxide of iron into peroxide, and precipitated by ammonia, gave peroxide of iron equal to three grains of metallic iron. The filtered solution was treated with pure potassa, and a hardly perceptible gray flocculent precipitate was obtained, so that this iron was free from nickel.

Carbon,	46.5
Iron,	3.0
Loss,	0.5

100.0

3. *Sulphuret of Iron*.—A small fragment of the pyrites was dissolved in diluted hydrochloric acid, under a brisk effervescence of

sulphuretted hydrogen gas. Part of it was insoluble; this after being washed and dried, was exposed to heat, by which the sulphur was sublimed, leaving a black powder. The quantity used was too small to determine the proportion; it is composed of *sulphuret of iron and carbon*.

4. *Hydroxide of Iron*.—The hydroxide of iron lost about 17 per cent. by being heated, and had all the characters of a similar residue from brown ironstone or hæmatite.

This is not the only instance in which meteoric iron has been found in the State of Tennessee. A small mass of it was found in Dickson County; another, a few miles west of Canyfork in De Kalb County. The latter had a smooth glossy surface, and was of an oval shape, its longer diameter being from 10 to 12 inches.

It is said that several masses have been found about 20 miles east from the warm springs in Buncombe County, North Carolina. I went to the spot, during my last excursion in East Tennessee, but I could learn nothing with certainty concerning it, and did not see any of the metal.*

Nashville, Tenn., Nov. 8, 1839.

XLVI.—*Observations on the Aurora Borealis of September 3, 1839; communicated by EDWARD C. HERRICK, Rec. Sec. Conn. Acad.*

On the night of Tuesday, the 3d of September, 1839, an extraordinary display of the Aurora Borealis was seen in all parts of the United States, and was probably also visible over a large portion of the northern hemisphere above the latitude of 30°. The public attention throughout the country was much attracted by this display, and numerous descriptions of its phenomena were published in the newspapers. I propose here to give a brief abstract of some of these accounts.

1. *New Haven*. Observations were made here by Mr. A. B. Haile, Mr. F. Bradley, and myself, and doubtless also by many others. The auroral light was first noticed about half an hour after sunset, and of course while the twilight was quite strong. At this time the sky was much obscured by thin clouds, but these gradually dispersed. As daylight faded, the Aurora grew more conspicuous, and soon presented a most splendid scene. So many good detailed

* One mass, at least, of meteoric iron has been found in this county, and an analysis of it was published by Professor C. U. Shepard, in this Jour., Vol. 36, p. 81.—EDS.

descriptions of great Auroral displays have however already been published in this Journal, that it seems unnecessary to attempt in this place a very minute account of the particulars of this instance. Previous to midnight, there were three or four seasons of maximum energy, during which a large portion of the heavens was covered with a vast assemblage of streamers of various hues, in which crimson and silver-white predominated. The exhibition was, on the whole, quite equal in splendour to any which we have ever seen in this region. Several times in the course of the evening, the corona was distinctly formed, enveloped, as usual in a tumultuous, ever-shifting mass of Auroral light. The mean of numerous observations of the altitude of the centre of the corona, taken by a plumb-quadrant, gave 74° ; which is not more than half a degree greater than the present magnetic dip at this place. Before 9h. 26m. there was but little undulation in the streamers, but about this time the Auroral waves began to show themselves, and soon flashed up towards the zenith with great magnificence. Low in the north, we saw at this time, what appeared to be short dark columns rising across the intensely luminous band which lay there, and then almost instantly vanishing. This was often repeated. The southern part of the heavens was occupied with streamers to very unusual extent. The arch bounding the streamers on the South gradually descended, so that at 10h. its vertex was not more than 10° above the horizon. We were consequently led to infer that this occurrence extended very far to the south of us, which has been found to be the fact.

At 7h. 37m. I stationed within the house, a surveyor's compass, so that the needle coincided with the N. and S. points. At 8h. 7m. it stood at 30° W. of N. At 9h. 7m. a splendid red blaze in the E., needle N. 30° E.: 9h. 27m. needle O. At noon on the 4th, the needle stood at N. $1^{\circ} 30'$ W., so that, (if we assume, as is most probable, that the needle had then regained its usual place,) its north end was not observed during the Aurora, to be carried to the west of its mean position. Circumstances rendered it inconvenient to retain the instrument any longer in the place it occupied during the Aurora, so that it cannot be confidently asserted that the influence of the Aurora was entirely ended at noon on the 4th. The needle is much less sensitive than that of the variation compass formerly employed. Of course I can not compare the magnetic effects of this display with those of other great occasions of this kind. During the evening, the temperature was from 70° to 60° . At 9h. the dew point was 58° , the air being 65° .

We discontinued our observations a little after 11h. at which time the display had greatly declined. A person who was abroad after midnight, informed me that about 1 A. M., (4th,) the spectacle was, if possible, more splendid than before. At 4 A. M., I found numerous streamers in active undulation, about the northern horizon; but not reaching to a greater elevation than 20° .

During the night of Wednesday, the 4th, the sky was densely overcast. A moderate Auroral display was seen at Albany, N. Y., at Middlebury, Vt., and probably at many other places where the state of the weather permitted observation.

2. *Nashville, Tenn.* N. lat. $36^{\circ} 9' 23''$; W. lon. $86^{\circ} 49'$. The following observations are contained in a letter to Prof. Silliman from Prof. James Hamilton, of the University of Nashville. "Although a resident in this city for more than six years, between 1827 and 1835, I have never seen so beautiful an exhibition of the *Aurora Borealis*, as that which occurred in the evening of September 3, 1839; nor have I been able to learn that any of the oldest inhabitants remember one of equal magnificence. The late display was but little inferior to those I have observed in New Jersey, three times within the last four years. About 7 P. M., the northern sky appeared unusually bright, as if affected by lunar twilight. A large bank of vapor or thin cloud was discovered in the N., gently declining towards the E. and W. points of the horizon, and extending perhaps 30° in each direction. A similar bank of smaller dimensions was seen in the N. E., but less bright than the Northern. In a few minutes, the upper edges of both banks, but especially of the Northern, had a whiteness resembling the enlightened disk of the moon in its greatest splendour. The bank was at this time about 12° in height as determined by a theodolite. At 7h. 25m. a white streamer about 2° in width, arose from the bank about N. 6° E., and shot upwards through the Pole star as far as the zenith, being rather convex on the Western side. Others appeared immediately on both sides of it, passing through or near *Ursa Major* and *Cassiopea*. The bank in the N. E. exhibited as yet no coruscations. At 7h. 45m., the columns had become larger and more numerous. One embraced *Ursa Major*, and another *Cassiopeia*, without farther extension horizontally. These were both of a brilliant crimson colour, and remained nearly stationary for a considerable time, while the intervening column became faint. A westward motion was soon after observed in three principal columns, and about the same time a diminution in the brightness of the red ones. On this last occurrence, thin horizontal clouds of a red colour were seen crossing the columns at an altitude of 30° , which however soon disappeared. When the Northern bank possessed less energy, the Northeastern sent up to the zenith an intensely red column, which continued to glow until nearly 9h., alternating however in brightness with those in the N. The Northern coruscations ceased about 9 P. M., and the other bank gave forth afterward but few and less vivid streamers. Before 10h. these had also ceased, yet the Northern bank continued to exhibit its silvery edge; and another display occurred after midnight. The Northern bank attained an altitude of $22\frac{1}{2}^{\circ}$ at 8h. 15m. and was about 3° higher at 8h. 40m.

"A very brilliant white column, 1° wide, and 3° W. of *Arcturus*, appeared to have undulations in the directions of its length for a considerable time, as if caused by the gentle flow of a fluid on its surface. The first column from the N. was evidently either in or very near the plane of the magnetic meridian. The banks continued to increase along the horizon until 9 P. M., when they extended from the E. to within 10° of the West. The extremities were not bright, but had the usual appearance of light clouds.

At this most westerly point, a shower had arisen about sunset which had been driven toward the S. W., and during the Aurora flashes of lightning at a great distance were occasionally seen. These became less and less vivid, the storm being driven away by a N. E. wind, blowing at the rate of three miles, and increasing to about six miles an hour during the phenomenon. The needle (about 10 inches long, and very sensitive,) vibrated through an arc of $1^{\circ} 10'$, while the columns were apparent, and on the following morning it had rested in an intermediate position only $20'$ W. of its greatest eastern limit during the evening. The plate of an electrical machine, in an adjoining room, showed more than usual activity, giving after two turns, pungent and loud sparks to the knuckles three inches distant. The season has been exceedingly dry since the middle of July.

"Taking the direction of my transit telescope, I found the magnetic variation this day, (Sept. 7, 1839,) to be $5^{\circ} 56' E.$ "

3. At *New Orleans, La.*, (N. lat. $29^{\circ} 58'$) the Auroral display was quite conspicuous, and appeared so much like a large conflagration, that the fire engines were called out to extinguish the flames. The attitude of the streamers is not mentioned. No corona was probably formed at this place. Being desirous to ascertain how far south a corona was visible, I made special inquiries of a friend at *Claiborne, Ala.*, (N. lat. $31\frac{1}{2}^{\circ}$) where the Aurora was very splendid, and learn that it could scarcely be said that a corona was formed there, although several times the Auroral columns were nearly united overhead. It is probable that a corona might have been seen within a hundred miles north of this.

4. Throughout *England* the Aurora of September 3rd, is described as most gorgeous. A confused and rhetorical account contained in a London paper of Sept. 4, (copied in N. Y. Journal of Commerce, Oct. 12,) states that the Aurora "had a most alarming appearance, and was exactly like that occasioned by a terrific fire. The consternation in the metropolis was very great, thousands of persons were running in the direction of the supposed awful catastrophe. * * * At two o'clock in the morning (4th) the phenomenon presented a most gorgeous scene, and one very difficult to describe. The whole of London was illuminated as light as noonday, (!) and the atmosphere was remarkably clear. The southern hemisphere though unclouded was very dark, but the stars which were innumerable shone beautifully. The opposite side of the heavens presented a singular, but magnificent contrast; it was clear in the extreme, and the light was very vivid. There was a continual succession of meteors which varied in splendour. They apparently formed in the centre of the heavens, and spread till they seemed to burst; the effect was electrical, myriads of small stars shot out over the horizon, and darted with that swiftness towards the earth that the eye could scarcely follow the track; they seemed to burst also, and throw a dark crimson vapour over the entire hemisphere. * * * Stars were darting about in all directions, and continued until 4 o'clock, when all died away."

Fig 1.

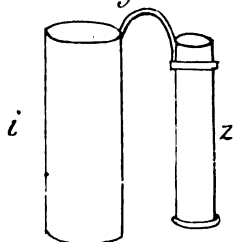


Fig 3.

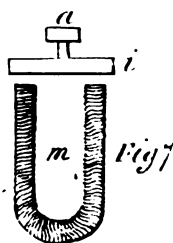
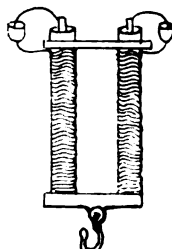


Fig 2.

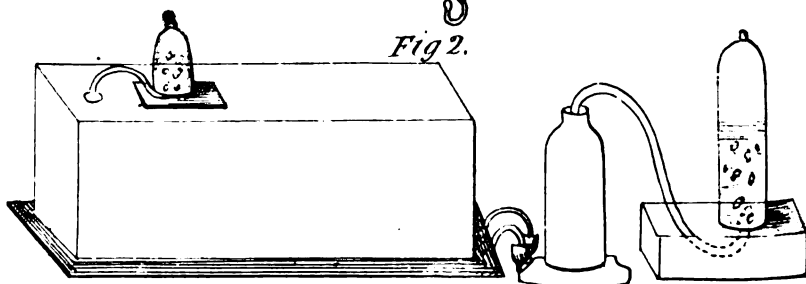


Fig 4.

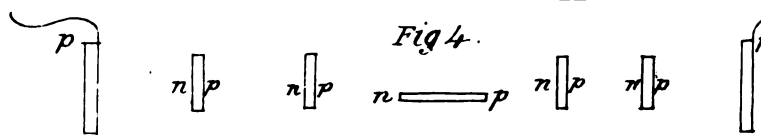


Fig 6.

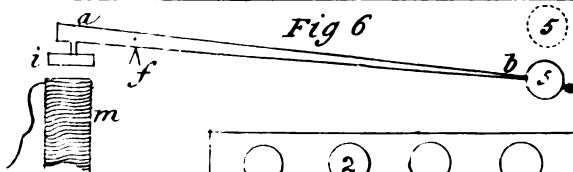


Fig 5.

	(2)			(5)	
1 - a	12 - h	23 - n	34 - r	45 - u	56 - s
2 - b	13 - i	24 - o	35 - s	46 - w	
3 - d	14 - k	25 - p	36 - t		
4 - c	15 - l	26 - q			
5 - f	16 - m				
6 - g					

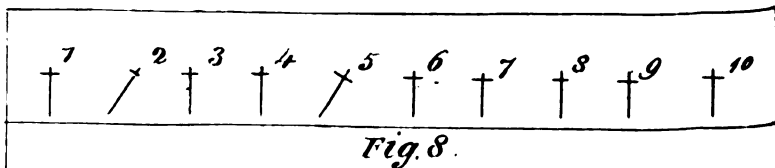
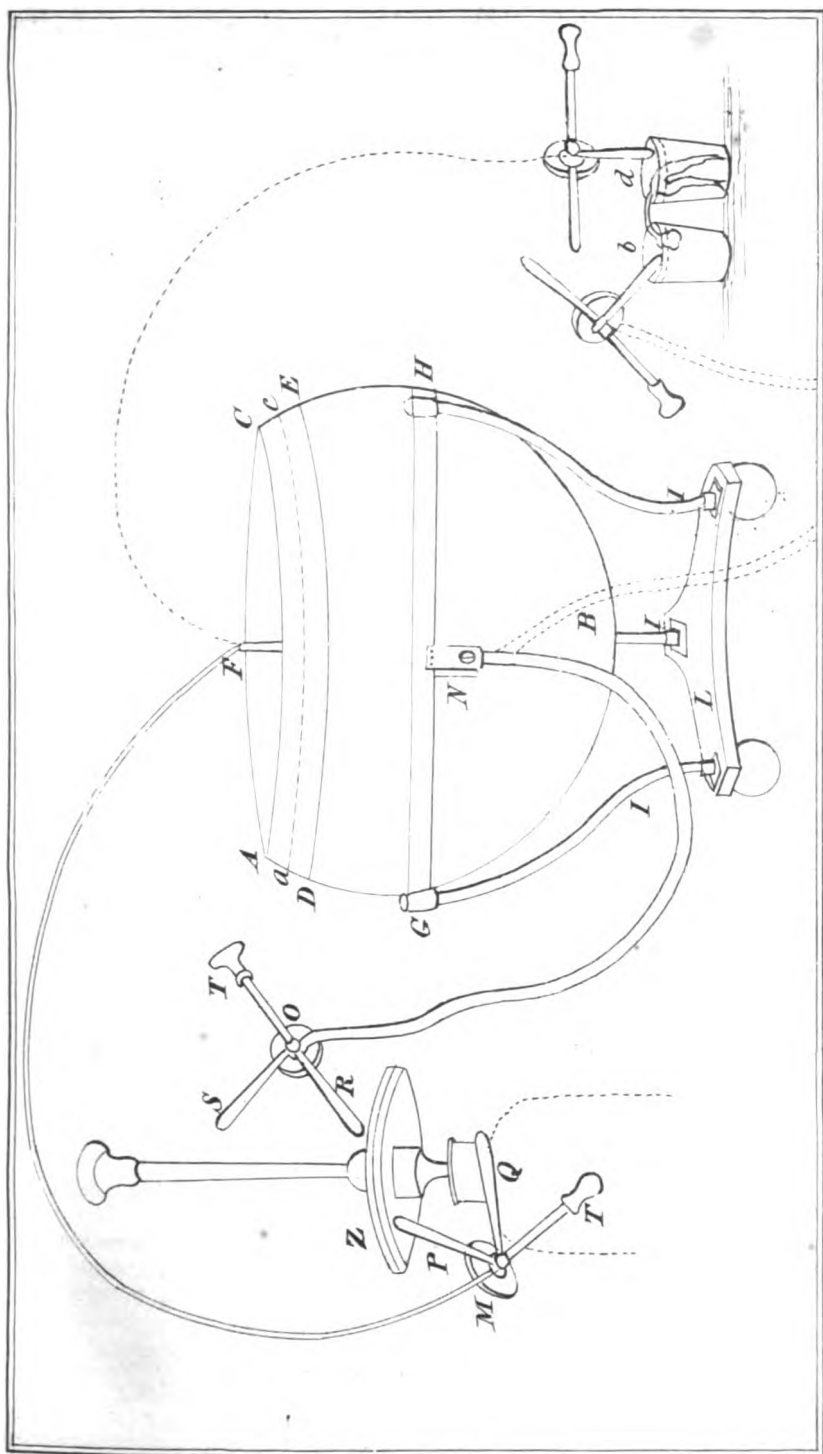


Fig 8.



THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND CHEMISTRY;

AND
Guardian of Experimental Science.

NOVEMBER, 1840.

XLVII.—Experimental Researches in Electricity.—Thirteenth Series. By MICHAEL FARADAY, Esq., D.C.L., F.R.S., Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acad. of Sciences, Paris, Petersburg, Florence, Copenhagen, Berlin, &c. &c.

Received February 22,—Read March 15, 1838.

¶ x. *Convection, or carrying discharge (continued).* ¶ xi. *Relation of a vacuum to electrical phenomena.* §. 19. *Nature of the electrical current.*

(Continued from page 284).

1610. The latter experiments (1609.) may therefore be considered as failing to give the hoped-for proof, but I have much confidence in the former (1605. 1608.), and in the considerations (1603.) connected with them. If I have rightly viewed them, and we may be allowed to relate the currents at points and surfaces in such extremely different bodies as air and the metals, and admit that they are effects of the *same* kind, differing only in degree and in proportion to the insulating or conducting power of the dielectric used, what great additional argument we obtain in favour of that theory, which in the phenomena of insulation and conduction also, as in these, would link *the same* apparently dissimilar sub-

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stances together (1336. 1561.); and how completely the general view, which refers all the phenomena to the direct action of the molecules of matter, seems to embrace the various isolated phenomena as they successively come under consideration !

1611. The connection of this convective or carrying effect, which depends upon a certain degree of insulation, with conduction ; i. e. the occurrence of both in so many of the substances referred to, as, for instance, the metals, water, air, &c., would lead to many very curious theoretical generalizations, which I must not indulge in here. One point, however, I shall venture to refer to. Conduction appears to be essentially an action of contiguous particles, and the considerations just stated, together with others formerly expressed (1326. 1336, &c.), lead to the conclusion, that all bodies conduct, and by the same process, air as well as metals ; the only difference being in the necessary degree of force or tension between the particles which must exist before the act of conduction or transfer from one particle to another can take place.

1612. The question then arises, what is this limiting condition which separates, as it were, conduction and insulation from each other ? Does it consist in a difference between the two contiguous particles, or the contiguous poles of these particles in the nature and amount of positive and negative force, no communication or discharge occurring unless that difference rises up to a certain degree, variable for different bodies, but always the same for the same body ? Or is it true that, however small the difference between two such particles, if *time* be allowed, equalization of force will take place, even with the particles of such bodies as air, sulphur or lac ? In the first case, insulating power in any particular body would be proportionate to the degree of the assumed necessary difference of force ; in the second, to the *time* required to equalize equal degrees of difference in different bodies. With regard to air, one is almost led to expect a permanent difference of force, but in all other bodies, time seems to be quite sufficient to ensure, ultimately, complete conduction. The difference in the modes by which insulation may be sustained, or conduction effected, is not a mere fanciful point, but one of great importance, as being essentially connected with the molecular theory of induction, and the manner in which the particles of bodies assume and retain their polarized state.

¶ xi. *Relation of a vacuum to electrical phenomena.*

1613. It would seem strange if a theory which refers all the phenomena of insulation and conduction, i. e. all electrical phenomena, to the action of contiguous particles, were to omit to notice the assumed possible case of a *vacuum*. Admitting that a vacuum can be produced, it would be a very curious matter indeed to know what its relation to electrical phenomena would be ; and as shell-lac and metal are directly opposed to each other, whether a

vacuum would be opposed to them both, and allow neither of induction or conduction across it. Mr. Morgan* has said that a vacuum does not conduct. Sir H. Davy concluded from his investigations, that as perfect a vacuum as could be made† did conduct, but does not consider the prepared spaces which he used as absolute vacua. In such experiments I think I have observed the luminous discharge to be principally on the inner surface of the glass; and it does not appear at all unlikely, that, if the vacuum refused to conduct, still the surface of glass next it might carry on that action.

1614. At one time, when I thought inductive force was exerted in right lines, I hoped to illustrate this important question by making experiments on induction with metallic mirrors (used only as conducting vessels) exposed towards a very clear sky at night time, and of such concavity that nothing but the firmament could be visible from the lowest part of the concave *n*, fig. 29, plate iii. Such mirrors, when electrified, as by connection with a Leyden jar, and examined by a carrier ball, readily gave electricity at the lowest part of their concavity if in a room; but I was in hopes of finding that, circumstanced as before stated, they would give little or none at the same spot, if the atmosphere above really terminated in a vacuum. I was disappointed in the conclusion, for I obtained as much electricity there as before; but on discovering the action of induction in curved lines (1231.), found a full and satisfactory explanation of the result.

1615. My theory, as far as I have ventured it, does not pretend to decide upon the consequences of a vacuum. It is not at present limited sufficiently, or rendered precise enough, either by experiments relating to spaces void of matter, or those of other kinds, to indicate what would happen in the vacuum case. I have only as yet endeavoured to establish, what all the facts seem to prove, that when electrical phenomena, as those of induction, conduction, insulation and discharge occur, they depend on, and are produced by the action of *contiguous* particles of matter, the next existing particle being considered as the contiguous one; and I have further assumed, that these particles are polarized; that each exhibits the two forces, or the force in two directions (1295. 1298.); and that they act at a distance only by acting on the *contiguous* and intermediate particles.

1616. But assuming that a perfect vacuum were to intervene in the course of the lines of inductive action (1304.), it does not follow from this theory, that the particles on opposite sides of such a vacuum could not act on each other. Suppose it possible for a positively electrified particle to be in the centre of a vacuum an inch in diameter, nothing in my present views forbids that the particle should act at the distance of half an inch on all the particles forming the inner superficies of the bounding sphere, and with a force consistent with the well-known law of the squares of the distance. But suppose the sphere of an inch were full of insulating matter,

* Philosophical Transactions, 1785, p. 272.

† Ibid. 1822, p. 64.

the electrified particle would not then, according to my notion, act directly on the distant particles, but on those in immediate association with it, employing *all* its power in polarizing them ; producing in them negative force equal in amount to its own positive force and directed towards the latter, and positive force of equal amount directed outwards and acting in the same manner upon the layer of particles next in succession. So that ultimately, those particles in the surface of a sphere of half an inch radius, which were acted on *directly* when that sphere was a vacuum, will now be acted on *indirectly* as respects the central particle or source of action, i. e. they will be polarized in the same way, and with the same amount of force.

§ 19. Nature of the electric current.

1617. The word *current* is so expressive in common language, that when applied in the consideration of electrical phenomena we can hardly divest it sufficiently of its meaning, or prevent our minds from being prejudiced by it (283. 511.). I shall use it in its common electrical sense, namely, to express generally a certain condition and relation of electrical forces supposed to be in progression.

1618. A current is produced both by excitement and discharge ; and whatsoever the variation of the two general causes may be, the effect remains the same. Thus excitement may occur in many ways, as by friction, chemical action, influence of heat, change of condition, induction, &c. ; and discharge has the forms of conduction, electrolyzation, disruptive discharge, and convection ; yet the current connected with these actions, when it occurs, appears in all cases to be the same. This constancy in the character of the current, notwithstanding the particular and great variations which may be made in the mode of its occurrence, is exceedingly striking and important ; and its investigation and development promise to supply the most open and advantageous road to a true and intimate understanding of the nature of electrical forces.

1619. As yet the phenomena of the current have presented nothing in opposition to the view I have taken of the nature of induction as an action of contiguous particles. I have endeavoured to divest myself of prejudices and to look for contradictions, but I have not perceived any in conductive, electrolytic, convective, or disruptive discharge.

1620. Looking at the current as a *cause*, it exerts very extraordinary and diverse powers, not only in its course and on the bodies in which it exists, but collaterally, as in inductive or magnetic phenomena.

1621. *Electrolytic action*.—One of its direct actions is the exertion of pure chemical force, this being a result which has now been examined to a considerable extent. The effect is found to be *constant* and *definite* for the quantity of electric force discharged (783, &c.) ; and beyond that, the *intensity* required is in relation

to the intensity of the affinity or forces to be overcome (904. 906. 911.). The current and its consequences are here proportionate; the one may be employed to represent the other; no part of the effect of either is lost or gained; so that the case is a strict one, and yet it is the very case which most strikingly illustrates the doctrine that induction is an action of contiguous particles (1164. 1343.).

1622. The process of electrolytic discharge appears to me to be in close analogy, and perhaps in its nature identical with another process of discharge, which at first seems very different from it, I mean *convection*. In the latter case the particles may travel for yards across a chamber; they may produce strong winds in the air, so as to move machinery; and in fluids, as oil of turpentine, may even shake the hand, and carry heavy metallic bodies about;* and yet I do not see that the force, either in kind or action, is at all different to that by which a particle of hydrogen leaves one particle of oxygen to go to another, or by which a particle of oxygen travels in the contrary direction.

1623. Travelling particles of the air can effect chemical changes just as well as the contact of a fixed platina electrode, or that of a combining electrode, or ions of a decomposing electrolyte (453.471); and in the experiment formerly described, where eight places of decomposition were rendered active by one current (469.), and where charged particles of air in motion were the only electrical means of connecting these parts of the current, it seems to me that the action of the particles of the electrolyte and of the air were essentially the same. A particle of air was rendered positive; it travelled in a certain determinate direction, and coming to an electrolyte, communicated its powers; an equal amount of positive force was accordingly acquired by another particle (the hydrogen), and the latter, so charged, travelled as the former did, and in the same direction, until it came to another particle, and transferred its power and motion, making that other particle active. Now, though the particle of air travelled over a visible and occasionally a large space, whilst the particle of the electrolyte moved over an exceedingly small one; though the air particle might be oxygen, nitrogen, or hydrogen, receiving its charge from force of high intensity, whilst the electrolytic particle of hydrogen had a natural aptness to receive the positive condition with extreme facility; though the air particle might be charged with very little electricity at a very high intensity by one process, whilst the hydrogen particle might be charged with much electricity at a very low intensity by another process; these are not differences of kind, as relates to the final discharging action of these particles, but only of degree; not essential differences which make things unlike, but such differences as give to things, similar

* If a metallic vessel three or four inches deep, containing oil of turpentine, be insulated and electrified, and a rod with a ball (an inch or more in diameter) at the end, have the ball immersed in the fluid whilst the end is held in the hand, the mechanical force generated when the ball is moved to and from the sides of the vessel will soon be evident to the experimenter.

in their nature, that great variety which fits them for their office in the system of the universe.

1624. So when a particle of air, or of dust in it, electrified at a negative point, moves on through the influence of the inductive forces (1572.) to the next positive surface, and after discharge passes away, it seems to me to represent exactly that particle of oxygen which, having been rendered negative in the electrolyte, is urged by the same disposition of inductive forces, and going to the positive platina electrode, is there discharged, and then passes away, as the air or dust did before it.

1625. *Heat* is another direct effect of the *current* upon substances in which it occurs, and it becomes a very important question, as to the relation of the electric and heating forces, whether the latter is always definite in amount.* There are many cases, even amongst bodies which conduct without change, which stand out at present from the assumption that it is ;† but there are also many which indicate that, when proper limitations are applied, the heat produced is definite. Harris has shown this for a given length of current in a metallic wire, using common electricity ;‡ and De la Rive has proved the same point for voltaic electricity by his beautiful application of Breguet's thermometer.§

1626. When the production of heat is observed in electrolytes under decomposition, the results are still more complicated. But important steps have been taken in the investigation of this branch of the subject by De la Rive|| and others ; and it is more than probable that, when the right limitations are applied, constant and definite results will here also be obtained.

1627. It is a most important part of the character of the current, and essentially connected with its very nature, that it is always the same. The two forces are everywhere in it. There is never one current of force or one fluid only. Any one part of the current may, as respects the presence of the two forces there, be considered as precisely the same with any other part ; and the numerous experiments which imply their possible separation, as well as the theoretical expressions which, being used daily, assume it, are, I think, in contradiction with facts (511, &c.). It appears to me to be as impossible to assume a current of positive or a current of negative force alone, or of the two at once with any predominance of one over the other, as it is to give an absolute charge to matter (1169. 1177.).

1628. The conviction of this truth, if, as I think, it be a truth, or on the other hand the disproof of it, is of the greatest conse-

* See De la Rive's *Researches*, Bib. Universelle, 1829, xl. p. 40.

† Amongst others, Davy, *Philosophical Transactions*, 1821, p. 438. Pelletier's important results, *Annales de Chimie*, 1834, lvi. p. 371. and Becquerel's non-heating current, Bib. Universelle, 1835, lx. 218.

‡ *Philosophical Transactions*, 1824, pp. 225. 228.

§ *Annales de Chimie*, 1836, lxii. 177.

|| Bib. Universelle, 1829, xl. 49 ; and Ritchie, *Phil. Trans.* 1832, p. 296.

quence. If, as a first principle, we can establish that the centres of the two forces, or elements of force, never can be separated to any sensible distance, or at all events not further than the space between two contiguous particles (1615.), or if we can establish the contrary conclusion, how much more clear is our view of what lies before us, and how much less embarrassed the ground over which we have to pass in attaining to it, than if we remain halting between two opinions! And if, with that feeling, we rigidly test every experiment which bears upon the point, as far as our prejudices will let us (1161.), instead of permitting them with a theoretical expression to pass too easily away, are we not much more likely to attain the real truth, and from that proceed with safety to what is at present unknown?

1629. I say these things not, I hope, to advance a particular view, but to draw the strict attention of those who are able to investigate and judge of the matter, to what must be a turning point in the theory of electricity; to a separation of two roads, one only of which can be right: and I hope I may be allowed to go a little further into the facts which have driven me to the view I have just given.

1630. When a wire in the voltaic circuit is heated, the temperature frequently rises first, or most at one end. If this effect were due to any relation of positive or negative as respects the current, it would be exceedingly important. I therefore examined several such cases; but when, keeping the contacts of the wire and its position to neighbouring things unchanged, I altered the direction of the current, I found that the effect remained unaltered, showing that it depended, not upon the direction of the current, but on other circumstances. So there is here no evidence of a difference between one part of the circuit and another.

1631. The same point, i. e. uniformity in every part, may be illustrated by what may be considered as the inexhaustible nature of the current when producing particular effects; for these effects depend upon transfer only, and do not consume the power. Thus a current which will heat one inch of platina wire will heat a hundred inches (853. note). If a current be sustained in a constant state, it will decompose the fluid in one voltameter only, or in twenty others if they be placed in the circuit, in each to an amount equal to that in the single one.

1632. Again, in cases of disruptive discharge, as in the spark, there is frequently a dark part (1422.), which, by Professor Johnson, has been called the neutral point*; and this has given rise to the use of expressions implying that there are two electricities existing separately, which, passing to that spot, there combine and neutralize each other†. But if such expressions are understood as correctly indicating that positive electricity alone is moving between the positive ball and that spot, and negative electricity only between the negative ball and that spot, then what strange conditions these

* Silliman's Journal, 1834, xxv. p. 57.

† Thomson on Heat and Electricity, p. 471.

parts must be in ; conditions, which to my mind are every way unlike that which really occurs ! In such a case, one part of a current would consist of positive electricity only, and that moving in one direction ; another part would consist of negative electricity only, and that moving in the other direction ; and a third part would consist of an accumulation of the two electricities, not moving in either direction, but mixing up together, and being in a relation to each other utterly unlike any relation which could be supposed to exist in the two former portions of the discharge. This does not seem to me to be natural. In a current, whatever form the discharge may take, or whatever part of the circuit or current is referred to, as much positive force as is there exerted in one direction, so much negative force is there exerted in the other. If it were not so we should have bodies electrified not merely positive and negative, but on occasions in a most extraordinary manner, one being charged with five, ten, or twenty times as much of both positive and negative electricity in equal quantities as another. At present, however, there is no known fact indicating such states.

1633. Even in cases of convection, or carrying discharge, the statement that the current is everywhere the same must in effect be true (1627.) : for how, otherwise, could the results formerly described occur ? When currents of air constituted the mode of discharge between the portions of paper moistened with iodide of potassium or sulphate of soda (465. 469.), decomposition occurred ; and I have since ascertained that, whether a current of positive air issued from a spot, or one of negative air passed towards it, the effect of the evolution of iodine or of acid was the same, whilst the reversed currents produced alkali. So also in the magnetic experiments (307.) whether the discharge was effected by the introduction of a wire, or the occurrence of a spark, or the passage of convective currents either one way or the other, (depending on the electrified state of the particles) the result was the same, being in all cases dependent upon the perfect current.

1634. Hence, the section of a current compared with other sections of the same current must be a constant quantity, if the actions exerted be of the same kind ; or if of different kinds, then the forms under which the effects are produced are equivalent to each other, and experimentally convertible at pleasure. It is in sections, therefore, we must look for identity of electrical force, even to the sections of sparks and carrying actions, as well as those of wires and electrolytes.

1635. In illustration of the utility and importance of establishing that which may be the true principle, I will refer to a few cases. The doctrine of unipolarity as formerly stated, and I think generally understood,* is evidently inconsistent with my view of a current (1627.) ; and the latter singular phenomena of poles and

* Erman, *Annales de Chimie*, 1807, lxi. p. 115. Davy's *Elements*, p. 168. Biot, *Ency. Brit. Supp.* iv. p. 444. Becquerel, *Traite*, i. p. 167. De la Rive, *Bib. Univ.* 1837, vii. 392.

flames described by Erman and others* partake of the same inconsistency of character. If a unipolar body could exist, i. e. one that could conduct the one electricity and not the other, what very new characters we should have a right to expect in the currents of single electricities passing through them, and how greatly ought they to differ, not only from the common current which is supposed to have both electricities travelling in opposite directions in equal amount at the same time, but also from each other! The facts, which are excellent, have, however, gradually been more correctly explained by Becquerel,† Andrews,‡ and others; and I understand that Professor Ohms§ has perfected the work, in his close examination of all the phenomena; and after showing that similar phenomena can take place with good conductors, proves that with soap, &c. many of the effects are the mere consequences of the bodies evolved by electrolytic action.

1636. I conclude, therefore, that the *facts* upon which the doctrine of unipolarity was founded are not adverse to that unity and indivisibility of character which I have stated the current to possess, any more than the phenomena of the pile itself, which might well bear comparison with those of unipolar bodies, are opposed to it. Probably the effects which have been called effects of unipolarity, and the peculiar differences of the positive and negative surface when discharging into air, gases, or other dielectrics (1480. 1525.) which have been already referred to, may have considerable relation to each other.||

1637. M. de la Rive has recently described a peculiar and remarkable effect of heat on a current when passing between electrodes and a fluid.¶ It is, that if platina electrodes dip into acidulated water no change is produced in the passing current by making the positive electrode hotter or colder; whereas making the negative electrode hotter increased the deflexion of a galvanometer affected by the current, from 12° to 30° and even 45° , whilst making it colder diminished the current in the same high proportions.

1638. That one electrode should have this striking relation to heat whilst the other remained absolutely without, seem to me as incompatible with what I conceived to be the character of a current as unipolarity (1627. 1635.), and it was therefore with some anxiety

* Erman, *Annales de Chimie*, 1824, xxv. 278. Becquerel, *Ibid.* xxxvi. p. 329.

† Becquerel, *Annales de Chimie*, 1831, xlv. p. 283.

‡ Andrews, *Philosophical Magazine*, 1836, ix. 182.

§ Schweigger's *Jahrbuch der Chemie*, &c. 1830. Heft 8. Not understanding German, it is with extreme regret I confess I have not access, and cannot do justice, to the many most valuable papers in experimental electricity published in that language. I take this opportunity also of stating another circumstance which occasions me great trouble, and, as I find by experience, may make me seemingly regardless of the labours of others:—it is a gradual loss of memory for some years past; and now, often when I read a memoir, I remember that I have seen it before, and would have rejoiced if at the right time I could have recollected and referred to it in the progress of my own papers.—M. F.

|| See also Hare in *Silliman's Journal*, 1833, xxiv. 246.

¶ *Bibliothèque Universelle*, 1837, vii. 388.

that I repeated the experiment. The electrodes which I used were of platina; the electrolyte, water containing about one sixth of sulphuric acid by weight: the voltaic battery consisted of two pairs of amalgamated zinc and platina plates in dilute sulphuric acid, and the galvanometer in the circuit was one with two needles, and gave when the arrangement was complete a deflexion of 10° or 12° .

1639. Under these circumstances heating either electrode increased the current; heating both produced still more effect. When both were heated, if either were cooled, the effect on the current fell in proportion. The proportion of effect due to heating this or that electrode varied, but on the whole heating the negative seemed to favour the passage of the current somewhat more than heating the positive. Whether the application of heat were by a flame applied underneath, or one directed by a blow pipe from above, or by a hot iron or coal, the effect was the same.

1640. Having thus removed the difficulty out of the way of my views regarding a current, I did not pursue this curious experiment further. It is probable, that the difference between my results and those of M. de la Rive may depend upon the relative values of the currents used; for I employed only a weak one resulting from two pairs of plates two inches long and half an inch wide, whilst M. de la Rive used four pairs of plates of sixteen square inches in surface.

1641. Electric discharges in the atmosphere in the form of balls of fire have occasionally been described. Such phenomena appear to me to be incompatible with all that we know of electricity and its modes of discharge. As time is an element in the effect (1418. 1436.) it is possible perhaps that an electric discharge might really pass as a ball from place to place; but as every thing shows that its velocity must be almost infinite, and the time of its duration exceedingly small, it is impossible that the eye should perceive it as anything else than a line of light. That phenomena of balls of fire may appear in the atmosphere, I do not mean to deny; but that they have anything to do with the discharge of ordinary electricity, or are at all related to lightning or atmospheric electricity, is much more than doubtful.

1642. All these considerations, and many others, help to confirm the conclusion, drawn over and over again, that the current is an indivisible thing; an axis of power, in every part of which both electric forces are present in equal amount* (517. 1627.). With conduction and electrolyzation, and even discharge by spark, such a view will harmonize without hurting any of our preconceived notions; but as relates to convection, a more startling result appears, which must therefore be considered.

* I am glad to refer here to the results obtained by Mr. Christie with magneto-electricity, *Philosophical Transactions*, 1833, p. 113. note. As regards the current in a wire, they confirm everything that I am contending for.

1643. If two balls A and B be electrified in opposite states and held within each other's influence, the moment they move towards each other, a current, or those effects which are understood by the word current, will be produced. Whether A move towards B, or B move in the opposite direction towards A, a current, and in both cases having the same *direction*, will result. If A and B move from each other, then a *current* in the opposite direction, or equivalent effects, will be produced.

1644. Or, as charge exists only by induction (1178. 1299.), and a body when electrified is necessarily in relation to other bodies in the opposite state; so, if a ball be electrified positively in the middle of a room and be then moved in any direction, effects will be produced, as if a *current* in the same direction (to use the conventional mode of expression) had existed: or, if the ball be negatively electrified, and then moved, effects as if a current in a direction contrary to that of the motion had been formed, will be produced.

1645. I am saying of a single particle or of two what I have before said, in effect, of many (1633.). If the former account of currents be true, then that just stated must be a necessary result. And, though the statement may seem startling at first, it is to be considered that, according to my theory of induction, the charged conductor or particle is related to the distant conductor in the opposite state, or that which terminates the extent of the induction, by all the intermediate particles (1165. 1295.), these becoming polarized exactly as the particles of a solid electrolyte do when interposed between the two electrodes. Hence the conclusion regarding the unity and identity of the current in the case of convection, jointly with the former cases, is not so strange as it might at first appear.

1646. There is a very remarkable phenomenon or effect of the electrolytic discharge, first pointed out, I believe, by Mr. Porrett, of the accumulation of fluid under decomposing action in the current on one side of an interposed diaphragm.* It is a mechanical result; and as the liquid passes from the positive towards the negative electrode in all the known cases, it seems to establish a relation to the polar condition of the dielectric in which the current exists (1164. 1525.). It has not as yet been sufficiently investigated by experiment; for De la Rive says,† it requires that the water should be a bad conductor, as, for instance, distilled water, the effect not happening with strong solutions; whereas, Dutrochet says‡ the contrary is the case, and that, the effect is not directly due to the electric current.

1647. Becquerel in his *Traité de l'Electricité* has brought together the considerations which arise for and against the opinion, that the effect generally is an electric effect.§. Though I have no

* *Annals of Philosophy*, 1816, viii. p. 75.

† *Annales de Chimie*, 1835, xxviii. p. 196.

‡ *Annales de Chimie*, 1832, xlix. p. 423.

§ Vol. iv. p. 197, 192.

decisive fact to quote at present, I cannot refrain from venturing an opinion, that the effect is analogous both to combination and convection (1623.), being a case of carrying due to the relation of the diaphragm and the fluid in contact with it, through which the electric discharge is jointly effected; and further, that the peculiar relation of positive and negative small and large surfaces already referred to (1482. 1503. 1525.), may be the direct cause of the fluid and the diaphragm travelling in contrary but determinate directions. A very valuable experiment has been made by M. Becquerel with particles of clay,* which will probably bear importantly on this point.

1648. *As long as* the terms *current* and *electro-dynamic* are used to express those relations of the electric forces in which progression of either fluids or effects are supposed to occur (283.), *so long* will the idea of velocity be associated with them; and this will, perhaps, be more especially the case if the hypothesis of a fluid or fluids be adopted.

1649. Hence has arisen the desire of estimating this velocity either directly or by some effect dependent on it; and amongst the endeavours to do this correctly, may be mentioned especially those of Dr. Watson† in 1748, and of Professor Wheatstone‡ in 1834; the electricity in the early trials being supposed to travel from end to end of the arrangement, but in the latter investigations a distinction occasionally appearing to be made between the transmission of the effect and of the supposed fluid by the motion of whose particles that effect is produced.

1650. Electrolytic action has a remarkable bearing upon this question of the velocity of the current, especially as connected with the theory of an electric fluid or fluids. In it there is an evident transfer of power with the transfer of each particle of the anion or cation present, to the next particles of the cation or anion; and as the amount of power is definite, we have in this way a means of localizing as it were the force, identifying it by the particle and dealing it out in successive portions, which leads, I think, to very striking results.

1651. Suppose, for instance, that water is undergoing decomposition by the powers of a voltaic battery. Each particle of hydrogen as it moves one way, or of oxygen as it moves in the other direction, will transfer a certain amount of electrical force associated with it in the form of chemical affinity (822. 852. 918.) onwards through a distance, which is equal to that through which the particle itself has moved. This transfer will be accompanied by a corresponding movement in the electrical forces throughout every part of the circuit formed (1627. 1634.), and its effects may be estimated, as, for instance, by the heating of a wire (852.) at any particular section of the current however distant. If the water be

* *Traite de l'Electricite*, i. p. 285.

† *Philosophical Transactions*, 1748.

‡ *Ibid.* 1834, p. 583.

a cube of an inch in the side, the electrodes touching, each by a surface of one square inch, and being an inch apart, then, by the time that a tenth of it, or 25·25 grains, is decomposed, the particles of oxygen and hydrogen throughout the mass may be considered as having moved relatively to each other in opposite directions, to the amount of the tenth of an inch ; i. e. that two particles at first in combination will after the motion be the tenth of an inch apart. Other motions which occur in the fluid will not at all interfere with this result ; for they have no power of accelerating or retarding the electric discharge, and possess in fact no relation to it.

1652. The quantity of electricity in 25·25 grains of water is, according to an estimate of the force which I formerly made (861.), equal to above 24 millions of charges of a large Leyden battery ; or it would have kept any length of a platina wire 1-104 of an inch in diameter red hot for an hour and a half (853.). This result, though given only as an approximation, I have seen no reason as yet to alter, and it is confirmed generally by the experiments and results of M. Pouillet.* According to Mr. Wheatstone's experiments the influence or effects of the current would appear at a distance of 576,000 miles in a second.† We have, therefore, in this view of the matter, on the one hand, an enormous quantity of power equal to a most destructive thunder storm appearing instantly at the distance of 576,000 miles from its source, and on the other, a quiet effect, in producing which the power had taken an hour and a half to travel through the tenth of an inch : yet these are the equivalents to each other, being effects observed at the sections of one and the same current (1634.).

1653. It is time that I should call attention to the lateral or transverse forces of the *current*. The great things which have been achieved by Oersted, Arago, Ampere, Davy, De la Rive, and others, and the high degree of simplification which has been introduced into their arrangement by the theory of Ampere, have not only done their full service in advancing most rapidly this branch of knowledge, but have secured to it such attention that there is no necessity for urging on its pursuit. I refer of course to magnetic action and its relations ; but though this is the only recognised lateral action of the current, there is great reason for believing that others exist and would by their discovery reward a close search for them (951.).

1654. The magnetic or transverse action of the current seems to be in a most extraordinary degree independent of those variations, or modes of action which it presents directly in its course ; it consequently is of the more value to us, as it gives us a higher relation of the power than any that might have varied with each mode of discharge. This discharge, whether it be by conduction through a wire with infinite velocity (1652), or by electrolyzation with its corresponding and exceeding slow motion (1651.), or by

* Becquerel, *Traite de l'Electricite*, v. p. 278.

† *Philosophical Transactions*, 1834, p. 589.

spark, and probably even by convection, produces a transverse magnetic action always the same in kind and direction.

1655. It has been shown by several experimenters, that whilst the discharge is of the *same kind* the amount of lateral or magnetic force is very constant (366. 367. 368. 376.). But when we wish to compare discharge of different kinds, for the important purpose of ascertaining whether the same amount of current will in its *different forms* produce the same amount of transverse action, we find the data very imperfect. Davy noticed, that when the electric current was passing through an aqueous solution it affected a magnetic needle*, and Dr. Ritchie says, that the current in the electrolyte is as magnetic as that in a metallic wire†, and has made water revolve round a magnet as a wire carrying the current would revolve.

1656. Disruptive discharge produces its magnetic effects: a strong spark, passed transversely to a steel needle, will magnetise it as well as if the electricity of the spark were conducted by a metallic wire occupying the line of discharge; and Sir H. Davy has shown that the discharge of a voltaic battery in vacuo is affected and has motion given to it by approximated magnets‡.

1657. Thus the three very different modes of discharge, namely, conduction, electrolyzation, and disruptive discharge, agree in producing the important transverse phenomenon of magnetism. Whether convection or carrying discharge will produce the same phenomenon has not been determined, and the few experiments I have as yet had time to make do not enable me to answer in the affirmative.

1658. Having arrived at this point in the consideration of the current and in the endeavour to apply its phenomena as tests of the truth or fallacy of the theory of induction which I have ventured to set forth, I am now very much tempted to indulge in a few speculations respecting its lateral action and its possible connection with the transverse condition of the lines of ordinary induction (1165. 1304.). I have long sought and still seek for an effect or condition which shall be to statical electricity what magnetic force is to current electricity; for as the lines of discharge are associated with a certain transverse effect, so it appeared to me impossible but that the lines of tension or of inductive action, which of necessity precede that discharge, should also have their correspondent transverse condition or effect (951.).

1659. According to the beautiful theory of Ampere, the transverse force of a current may be represented by its attraction for a similar current and its repulsion of a contrary current. May not then the equivalent transverse force of static electricity be represented by that lateral tension or repulsion which the lines of inductive action appear to possess (1304.)? Then again, when current

* Philosophical Transactions, 1821, p. 426.

+ Ibid. 1832, p. 294.

‡ Philosophical Transactions, 1821, p. 427.

or discharge occurs between two bodies, previously under inductive relations to each other, the lines of inductive force will weaken and fade away, and, as their lateral repulsive tension diminishes, will contract, and ultimately disappear in the line of discharge. May not this be an effect identical with the attractions of similar currents? i. e. may not the passage of static electricity into current electricity, and that of the lateral tension of the lines of inductive force into the lateral attraction of lines of similar discharge, have the same relation and dependencies, and run parallel to each other?

1660. The phenomena of induction amongst currents which I had the good fortune to discover some years ago (G. & C. 1048.) may perchance here form a connecting link in the series of effects. When a current is first formed, it tends to produce a current in the contrary direction in all the matter around it; and if that matter have conducting properties and be fitly circumstanced, such a current is produced. On the contrary, when the original current is stopped, one in the same direction tends to form all around it, and, in conducting matter properly arranged, will be excited.

1661. Now though we perceive the effects only in that portion of matter which, being in the neighbourhood, has conducting properties, yet hypothetically it is probable, that the non-conducting matter has also its relations to, and is affected by, the disturbing cause, though we have not yet discovered them. Again and again the relation of conductors and non-conductors has been shown to be one not of opposition in kind, but only of degree (1334. 1603.), and, therefore, for this, as well as for other reasons, it is probable, that what will affect a conductor will affect an insulator also; producing perhaps what may deserve the term of the electrotonic state (60. 242. 1114.).

1662. It is the feeling of the necessity of some lateral connexion between the lines of electric force (1114.); of some link in the chain of effects as yet unrecognised, that urges me to the expression of these speculations. The same feeling has led me to make many experiments on the introduction of insulating dielectrics having different inductive capacities (1270. 1277.) between magnetic poles and wires carrying currents, so as to pass across the lines of magnetic force. I have employed such bodies both at rest and in motion, without, as yet, being able to detect any influence produced by them; but I do by no means consider the experiments as sufficiently delicate, and intend, very shortly, to render them more decisive.

1663. I think the hypothetical question may at present be put thus: can such considerations as those already generally expressed (1658.) account for the transverse effects of electrical currents? are two such currents in relation to each other merely by the inductive condition of the particles of matter between them, or are they in relation by some higher quality and condition (1654.), which, acting at a distance and not by the intermediate particles, has, like the force of gravity, no relation to them?

1664. If the latter be the case, then, when electricity is acting upon and in matter, its direct and its transverse action are essentially

different in their nature ; for the former, if I am correct, will depend upon the contiguous particles, and the latter will not. As I have said before, this may be so, and I incline to that view at present, but I am desirous of suggesting considerations why it may not, that the question may be thoroughly sifted.

1665. The transverse power has a character of polarity impressed upon it. In the simplest forms it appears as attraction or repulsion, according as the currents are in the same or different directions : in the current and the magnet it takes up the condition of tangential forces ; and in magnets and their particles produces poles. Since the experiments have been made which have persuaded me that the polar forces of electricity, as in induction and electrolytic action (1298. 1343.), show effects at a distance only by means of the polarized contiguous and intervening particles, I have been led to expect that *all polar forces* act in the same general manner ; and the other kinds of phenomena which one can bring to bear upon the subject seem fitted to strengthen that expectation. Thus in crystallizations the effect is transmitted from particle to particle ; and in this manner, in acetic acid or freezing water a crystal a few inches or even a couple of feet in length will form in less than a second, but progressively and by a transmission of power from particle to particle. And, as far as I remember, no case of polar action, or partaking of polar action, except the one under discussion, can be found which does not act by contiguous particles.* It is apparently of the nature of polar forces that such should be the case, for the one force either finds or develops the contrary force near to it, and has, therefore, no occasion to seek for it at a distance.

1666. But leaving these hypothetical notions respecting the nature of the lateral action out of sight, and returning to the direct effects, I think that the phenomena examined and reasoning employed in this and the two preceding papers tend to confirm the view first taken (1164), namely, that ordinary inductive action and the effects dependent upon it, are due to an action of the contiguous particles of the dielectric interposed between the charged surfaces or parts which constitute, as it were, the terminations of the effect. The great point of distinction and power (if it have any) in the theory is, the making the dielectric of essential and specific importance, instead of leaving it as it were a mere accidental circumstance or the simple representative of space, having no more influence over the phenomena than the space occupied by it. I have still certain other results and views respecting the nature of the electrical forces and excitation, which are connected with the present theory ; and, unless upon further consideration they sink in my estimation, I shall very shortly put them into form as another series of these electrical researches.

Royal Institution, Feb. 4, 1838.

* I mean by contiguous particles those which are next to each other, not that there is no space between them. (See 1616.)

XLVIII.—Professor Wheatstone's Electro-Magnetic Telegraph.

Examination of Professor Wheatstone, and Charles Alexander Saunders, Esq., Secretary to the Great Western Railway, by the PARLIAMENTARY COMMITTEE ON RAILWAYS.

PROFESSOR WHEATSTONE EXAMINED.

Mr. Loch.—You have turned your attention for some time to the means of communicating intelligence by means of wires, by electricity? I have.

You have tried experiments to that effect to a considerable extent, have you not? I have been engaged in this enquiry for some years past, and in conjunction with a gentleman, Mr. Cooke, who has turned his attention to the same subject, I have within that time taken out several patents for the means of effecting this object, and the experiments have since been carried to a considerable extent on the Great Western Railway.

It is the continuity of property a railway possesses between two extreme points, which enabled you to try it more effectually? A railway offers considerable facilities; but the only necessary condition is, that a communication must be formed between two distant places by metallic wires.

Therefore a railway, affording the opportunity by means of continuous property between these two extreme points, enables the experiment to be tried better on a railway than where there is a variety of distinct properties intervening? A railway offers greater facilities, because greater attention can be paid to it.

Will you have the goodness to describe to the committee the mode in which you propose to communicate intelligence between two distant points, as alluded to by you? I have here a copy of the drawing of the specification to the first patent taken out by myself and Mr. Cooke; in all essential particulars the instrument here represented resembles the one at the Great Western Railway. Here is what may be called a dial (fig. A), with five vertical magnetic needles. Upon this dial 20 letters of the alphabet are marked, and the various letters are indicated by the mutual convergence of two needles when they are caused to move; if the first needle turns to the right, and the second to the left, H is indicated. If the first needle deviates to the right, and the fourth to the left, then B is indicated; if the same needles converge downwards, then V is pointed to. These magnetic needles are acted upon by electrical currents, passing through coils of wire placed immediately behind them; here is the representation of one of those coils, with the position of the magnetic needle with respect to it (fig. 6.). Each of the coils forms a portion of a communicating wire, which may extend to any distance whatever; these wires, at their termination, are connected with an apparatus, which may be called a communicator (fig. H.),

because by means of it the signals are communicated ; it consists of five longitudinal and two transverse metal bars, fixed in a wooden frame ; the latter are united to the two poles of a voltaic battery, and, in the ordinary condition of the instrument, have no metallic communication with the longitudinal bars, which are each immediately connected with a different wire of the line ; on each of these longitudinal bars two stops are placed, forming together two parallel rows. When a stop of the upper row is pressed down, the bar upon which it is placed forms a metallic communication with the transverse bar below it, which is connected with one of the poles of the battery ; and when one of the stops of the lower row is touched, another of the longitudinal bars forms a metallic communication with the other pole of the voltaic battery, and the current flows through the two wires connected with the longitudinal bars, to whatever distance they may be extended, passing up one and down the other, provided they be connected together at their opposite extremities, and affecting magnetic needles placed before the coils which are interposed in the circuit.

Both of these are at the London end ? Yes, both the communicator and the dial.

At each end there is that apparatus ? Yes ; there must be a similar complete apparatus at every different station.

Lord Granville Somerset.—This telegraph now extends from London to Drayton ? Yes.

Is not there a power at each station of communicating and being communicated to from every other station along the whole line ? There is.

Chairman.—Is there this apparatus at every station ? There may be one at each station if thought proper.

Putting up these at the intermediate stations does not injure it at the ends ? No, it will only require a slight addition to the number of elements of the voltaic battery in proportion to the number of intermediate stations.

Lord Granville Somerset.—How do you provide for the case of parties at different stations wishing to communicate at the same moment ? They cannot do so in the same line ; the one party must not work his telegraph if he sees it transmitting signals from another station.

Then, before he begins working, he must observe that there is no working at another part of the line ? He will know that by his own instrument, for all are at work at the same moment.

Suppose a party wishes to telegraph from Drayton to London at the same moment that a party at Hanwell wishes to telegraph to London, will the telegraph of the person at Drayton interfere with the telegraph of the person at Hanwell ? Two distinct messages cannot be sent through the same line at the same time from different stations.

Suppose the parties go unconsciously but simultaneously to the telegraph at the same moment ? They could not go unconsciously, because they would see that the apparatus was working.

Therefore they must wait till that working has ceased before they begin to send up their own message? Yes.

Mr. Loch.—Suppose there is a message intended for the extreme end, would the persons at each of those stations be made acquainted with the message intended for the extreme end? If the same telegraphic dictionary, or if the ordinary alphabetic spelling be employed at all the stations, every person on the watch would know what was going on; but if communication by cypher be adopted, a different one may be used at each station.

Lord Granville Somerset.—Suppose at the Drayton station there is an intimation desired to be given of the necessity of sending down an engine, will the parties at the other stations, be aware of that message being sent up? If there is only one line laid down, they would; at present the line consists of six wires, and one telegraph only is worked by their means.

Will the parties at each station know what is communicated from one point to another? Yes; unless the communication is sent in cypher.

Suppose it is sent in the ordinary way, without cypher or intended concealment, would it be known along the line? Yes, it might be read at all the stations simultaneously.

They are worked all the way along? Yes. There is another very essential part of the apparatus I wish to mention, which is, the means we have of ringing a bell before the communication begins, in order to call the attention of the observer; these drawings represent the mode first adopted, but other constructions are now in use. The general principle of the alarm here represented is this: to the detent of an alarm, on the ordinary construction of a clock alarm, a piece of soft iron is fixed, and opposite to it there is a bar of soft iron bent to the form of a horse-shoe; round this bent bar, wire, covered with silk, is wound, forming numerous coils; it is a property of soft iron to become powerfully magnetic when an electric current passes through a coil thus surrounding it. When the horse-shoe bar thus becomes magnetic, it therefore attracts the detent, and the bell immediately rings; when the current ceases the magnetic power ceases also, and the bell discontinues to ring. There are several other contrivances made to effect this purpose. Some arrangements are here represented to which Mr. Cooke has particularly directed his attention; they relate to the means of establishing communications at intermediate parts of the line where no fixed stations exist. To effect this, posts are placed at every quarter of a mile along the line, for the purpose of establishing a temporary communication with either of the adjacent stations; the guard of a train may thus carry with him a portable instrument, by means of which he can send up a message to a station either way, whenever it may be required. This is a representation of the mode by which this purpose is effected; here is one of the quarter of a mile posts: the wires are carried up through it, and there is, on the top of it, an apparatus to which the portable telegraph may be temporarily fixed, and by means of which a message may be sent in either direction of the line at pleasure.

Mr. Loch.—How are those wires kept insulated in the tubes? First, the wires are insulated from each other by a mixture of cotton and india rubber, which is a very good insulating material; then, these prepared wires are all passed, with certain precautions, through an iron tube, which in some parts of the line is buried beneath the ground, and in other parts of the line is raised above it.

That mixture of cotton and india-rubber cuts off all communication between the wire and the tube?—Yes, and between the separate wires; it is a sufficient non-conductor.

Chairman.—You say, a guard may communicate by means of one of these posts put up, with any station? With the stations either way.

He must carry a portable apparatus? Yes.

That must be the nature of the keys, must it not? The telegraphic apparatus necessarily consists of two parts, the communicating keys and the dial on which the indicated characters are seen; that which the guard carries must contain both these parts here; the keys and the dial are in the same apparatus.

Lord Granville Somerset.—Suppose the Great Western Railway were completed between London and Bristol, do you contemplate the possibility of carrying your telegraph through the whole way, so as to signify from London to Bristol anything you wish to communicate, and *vice versa* from Bristol to London? The experiment has not been tried, but I have every reason to believe that it can be done.

You must multiply your power considerably in that case; but if you can multiply your power sufficiently, there is no difficulty, in your opinion, in performing that? One very important circumstance I have ascertained is the little power requisite to produce this effect; it was formerly thought that to send a current to any considerable extent, very strong batteries must be employed, but in fact a very weak battery is sufficient, provided only it consists of a number of elements proportionate to the distance.

Do you see any practical difficulty in proportioning the number of your batteries to that extent, of 100, or 120 miles? I think there is none.

So far as your experiments have gone, you think you should be able to effect this telegraphic communication between Bristol and London? Yes; possibly several stations may be required, but, at any rate, the stations may be at far greater distances from each other than would be required for any ordinary system of telegraphs; my opinion is, that the intermediate stations will not be required.

You think you may communicate to the Reading station, the Reading station to another in the direction of Bristol, and that to Bristol? Yes; this means would be adopted if it should be found impracticable to effect an immediate communication between the two extreme stations.

Mr. French.—Have you any doubt you could do it with one intermediate station, dividing the distance? The experiment has

not been tried; if perfect insulation of the wires can be obtained, there will be no difficulty; theoretically there is no difficulty, but we might meet with practical obstacles in so long a line.

Lord Granville Somerset.—How long has this line been laid down upon the Great Western? I think it was finished in July last. (1839.)

Do you think you have had experience enough during the last winter to ascertain that it will not fail you in consequence of any inclemency of weather, or circumstances of that nature? If the wires are properly protected, I think there is no fear whatever.

Do you conceive they can be so protected, that weather will have no effect upon them? Yes, that is my judgment, from experiment.

Mr. Loch.—Is there any appreciable loss of time in making a communication from the Paddington station to the extremity of the line to which the telegraph is now carried? From some experiments I made some years ago, published in the *Philosophical Transactions*, when I first turned my attention to the possibility of effecting telegraphic communications, I ascertained that electricity travelled through a copper wire at the rate of about 200,000 miles in a second; consequently there is no appreciable time lost in the communication of the electrical effect; the only time that would be lost would be at relay stations, if they were necessary.

Mr. Freshfield.—Suppose you want to communicate from London to Bristol, how do you signify that your intention is to communicate with Bristol and not with Drayton? There would be a separate signal appropriated to each station, which would be made before the communication begins, immediately after the alarm has been rung.

Mr. Loch.—What is the rate at which light travels? 192,000 miles in a second.

What you described in the first instance is the mode of asking the question; how is the message received? The person who is attending reads the message from the dial.

Chairman.—Have the applications you have had from foreign countries to put up this means of communication been in connection with railways, or separate from railways? All in connection with railways.

Is it of any consequence that it should be on a railway, or does a railway offer any advantage in that respect? Not the slightest advantage with regard to laying down the line, but a great one with respect to its protection from injury.

Sir John Guest.—Have you tried to pass the line through water; There would be no difficulty in doing so, but the experiment has not yet been made.

Chairman.—Could you communicate from Dover to Calais in that way? I think it perfectly practicable.

Have you any further observations to make? An electrical telegraph offers a great many advantages over an ordinary telegraph; it will work day and night, but an ordinary telegraph will act only

during day; it will also work in all states of weather, an ordinary telegraph can only work in fine weather. There are a great number of days in the year in which no communication can be given by an ordinary telegraph, and, besides, a great many communications are stopped before they can be finished, on account of changes in the state of the atmosphere. No inconveniences of this kind would attend the electrical telegraph. Another advantage is, that the expense of the separate stations is by no means comparable to that of the ordinary telegraph; no look-out-men are required, and the apparatus may be worked in any room where there are persons to attend to it. There is another advantage the electric possesses over the ordinary telegraph. *viz.* the rapidity with which the signals may be made to follow each other. Thirty signals may be conveniently made in a minute; that number cannot be made by the ordinary telegraph. There is one thing I will take the opportunity to mention: I have been confining the attention of the committee to the telegraph now working on the Great Western Railroad, but having lately occupied myself in carrying into effect numerous improvements which have suggested themselves to me, I have, conjointly with Mr. Cooke, who has turned his attention greatly to the same subject, obtained a new patent for a telegraphic arrangement, which I think will present very great advantages over that which at present exists. It can be applied without entailing any additional expense of consequence to the line now laid down; it will only be necessary to substitute the new for the former instruments. This new apparatus requires only a single pair of wires to effect all which the present one does with five, so that three independent telegraphs may be immediately placed on the line of the Great Western; it presents in the same place all the letters of the alphabet according to any order of succession, and the apparatus is so extremely simple, that any person without any previous acquaintance with it can send a communication and read the answer. This apparatus I shall be happy to show the committee in action at King's College.

Sir John Guest.—Does not the possibility of cutting off the communication between one point and another, occur to you? The same objection may be made with regard to railroads themselves.

Suppose any person were to stop the communication from one town to another? By destroying the continuity of the rails they might stop the passage of the trains.

The common telegraphic communication could be kept up notwithstanding that? Certainly.

Chairman.—Can you state the expense which would be incurred in laying it down? I am hardly prepared to state that, because only one line has been laid down at present.

Mr. Loch.—Would it be your view ultimately, supposing the railway completed to Bristol, that there should be one line to telegraph to and another from Bristol? No; the same line will serve both purposes.

There will be no inconvenience in practice in making use of the same line? No.

Mr. H. Baring.—This sort of telegraph is not in operation on any other railroad? The Blackwell Company shortly intend to have it.

CHARLES ALEXANDER SAUNDERS, ESQ., CALLED IN AND EXAMINED.

Lord Granville Somerset.—As secretary of the Great Western Railroad Company, can you state to the committee whether they have adopted Mr. Wheatstone's magnetic telegraph? As far as West Drayton, 13 miles.

How long has it been adopted? It was finished in July last; it has been in operation about seven or eight months.

Was that laid down at the expense of the railroad? It was laid down under an agreement with Mr. Cooke, who is one of the patentees.

Was it on behalf of Mr. Wheatstone also? It was under agreement with Mr. Cooke, Mr. Cooke being the co-patentee.

Does that agreement extend to any length of time, or has this been only an experiment? The agreement contemplated the further extension of it, if the Company required it within a certain period of time, after the completion of the first 13 miles.

In fact the Company have laid down this magnetic telegraph at their own expense, under a specific agreement with Mr. Cooke, the Company taking the expense on the one hand and deriving any benefit they may derive on the other? Yes, just so.

That agreement is determinable at a certain period? It is.

Is it renewable at the option of either party? No; I think it is absolutely determinable, not renewable.

Have you any objection to state the term of years? I have no objection to state the substance of the agreement, but it is very long and very intricate; the material substance of it is this: that within a certain number of months after the telegraph shall have been laid and efficiently worked between Paddington and Drayton, the company might call upon the patentees to give them a license for the whole line, on certain terms; there are a variety of further considerations involved in the agreement, which it would be very difficult to relate.

Is this agreement binding upon the patentees, so as to enable the Great Western Company to execute this telegraph all the way from Bristol to London, for a certain number of years? It was binding upon them, but the time has now expired.

A new arrangement must be made before any permanent agreement is effected? Yes, neither party is now bound by that agreement.

Have all the advantages which were anticipated from this telegraph accrued? I think we have scarcely had it in a state to say that we have derived all the advantages which were contemplated from it, because we have, between West Drayton and Paddington, very little inducement to work the telegraph separately for that part; it had much more reference to the more distant stations, and the communications of our line with others, or to communications between places on the line where short and long trains together are

running upon the same portion of railroad. As yet we have had no practical benefit of that description, but it has enabled us to ascertain that the telegraph perfectly performs all the duty that was expected of it; as far as it goes, it works perfectly true.

Provided it shall work as well when your line is completed, do you anticipate all those useful results that were anticipated before it was laid down? I do, indeed.

That is your opinion, after your experience of eight or nine months on 13 miles? Yes.

In general terms, is it a very expensive thing to lay down this magnetic telegraph? It is expensive, but that is a question of degree: I have no objection to state the expense incurred; I believe it may be laid at from £250. to £300. a mile, including the charge for station instruments.

In the discussions which have taken place, of which you may have been cognizant, upon the subject of railroad telegraphs, have the directors contemplated the conveyance of ordinary articles of intelligence between Bristol and London? I think that view was entertained by the company when they originally tried it; the object would be to facilitate all means of communication.

Do you consider that that would be the only means by which the company would be remunerated for the outlay? I think the usefulness of it to the railway itself is the chief remuneration; it is calculated undoubtedly to simplify the working of the railway, and to diminish the stock of every description, whether of engines or of carriages; to insure greater punctuality, and, in cases of accident, to repair the injury with the least delay, as well as to produce general advantages and greater security in working the railway.

You think you might have a less establishment, and less stock, in consequence of having this magnetic telegraph, than you otherwise would be obliged to keep up to conduct the line? Undoubtedly.

And that in that way the company would be remunerated? I think that would be a mode of remuneration; I do not say to what extent it would operate, as compared with the expense of the telegraph itself.

In addition to the remuneration thus derived, do you conceive it will be an effectual mode of assisting in case of accident to passengers? Certainly, it would be so.

And in some instances of preventing accidents? Yes; if a line were at any place stopped up, and a communication could be made by telegraph, it would prevent the danger of collision from a subsequent train running up to the place of danger.

Mr. Wheatstone has stated that it is intended that a guard should have a portable telegraph, capable of operating at the distance of every quarter of a mile? Yes, that is a plan proposed; it has not been carried into effect on our line at present; there are places to which the portable telegraphs may be applied, but the men have not been instructed in it yet.

Supposing that idea were carried out, would it not be the cause of great safety in case of sudden emergencies, or fear of accidents? Yes; I have no doubt security against accidents would result, and more prompt assistance in case of accident.

Suppose an engine unexpectedly became unfit for service, have you not, in the course of the last few months, occasionally sent to another station for another engine, by means of the telegraph? Yes, we have, on one or two occasions within a few months; we worked the telegraph for nearly two months, so as to communicate to Paddington the moment of the passing of the train at West Drayton and Hanwell; that was done for the purpose of trying whether the telegraph would constantly work, and whether we could rely upon it, and it answered the purpose, certainly, admirably.

Do you contemplate continuing that constant use of it? No, we do not work it in that way; but it is used in any emergency; they can transmit any intelligence between West Drayton and Paddington, which it may be material to receive.

If parties will wait horses when they come to the Paddington station, you are in the habit of sending on intelligence of that? Yes, we are; I think the chief use of the telegraph, what I consider the chief advantage of it, would be upon the junction of two lines, where they are to be worked by the engines of one line; for instance, upon the line from Bristol to London, at the junction of the Cheltenham Railway, it would be a very great facility indeed if it could be ascertained at the moment at which the train comes up from Bristol which is to receive the Cheltenham traffic, that the Cheltenham train is on its progress, and either within five minutes or not within five minutes of the place; by that means there would be no useless delay to either train, and in the same manner the down train coming up would be able to send previous intelligence from a station, by which the engine from the Cheltenham train would be ready at any time to take the train on without any loss of time.

It would also, in case of any want of exactness in the arrival of a train, prevent collision, would it not? It would, and it would reduce the expense of working the line; the superintendent might be enabled, in many cases, by delaying the train only a few minutes, to save the expense of a second engine being sent for a long distance.

In case of any severe fogs in any particular district, would it not be a great advantage that the trains coming into that district should be made aware of that circumstance? Yes, I think in the case of our working short trains, which we shall probably do from Slough to London, independently of the Bristol trains, it would be very important for us to know at Paddington when a train is approaching, whether it be a Slough or a Bristol train, and for those at Slough to know that the long train is coming up, and is within a certain distance, or not within a certain distance, that they may prepare accordingly, whether to send on that train from Slough to London, or delay it for a short time.

Suppose you wish to send an extra train from one point of your line to another, without any means of communication, there must be always a certain degree of danger either of running into another train, or meeting another train? Yes, we are always obliged

to allow a certain interval to elapse before another train is sent.

That is not always a certain means of preventing collision, is it? No, it is not.

By means of this telegraph, could not you guard against the danger of accident in that respect? Undoubtedly, it would tend to security in those cases.

Mr. Loch.—Would not the possession of such a means of conveyance, after the telegraph is completed as far as Bristol, give the possessor of the telegraph a great advantage in a commercial point of view over the rest of the public? It might do so, if they should choose so to avail themselves of their property.

Has it ever occurred to you what remedy the public might have under those circumstances? I do not see how they possibly could have any remedy at all; I do not see why they ought to have any remedy.

Would it be unfair, under those circumstances, that the Railway Company should give facilities to other parties to erect other telegraphs along their lines, paying the company for such facilities? I think the company would not object to other parties having a facility if they were sufficiently paid for it; but I cannot conceive if a party possesses property, why he should refuse to make it useful to himself, or why he should be called upon to make it as useful to another as to himself.

Take the railway to Portsmouth, would it be at all a matter that would be indifferent to the country, that the directors of that railroad should have the means of communicating by means of their telegraph with London, while the Government is deprived of all communication between the principal naval station and the capital in the same manner? I think the case cannot arise; Government will have the power of course, if they choose to pay for it, of putting a telegraph of their own between Portsmouth and London; and there is no telegraph which could exist, whether on the Southampton or any other railway company possessing which would prevent the Government having the use of it, if they choose to pay for it, Government might have one, of course, if they would go to the expense of making it.

What expense do you refer to? The expense of buying land and putting it down.

Would it not be a much more ready way to give the Government the power to lay down the telegraph on the railway itself? Paying for it, I do not see the slightest objection to it.

Lord Granville Somerset.—Suppose a restriction of the advantages of the railway company to that which may be called their own peculiar business, and not allowing it to transmit other intelligence? It strikes me that that would be a prohibition to the company laying it down at all.

Mr. Loch.—You think if this rule were laid down, that all the intelligence of those who telegraph should be made public with the exception of that on their own affairs, that would operate as a pro-

hibition to their laying it down at all? I scarcely know as to any rule of its being made public; I am answering these questions very much in the dark, but it strikes me that saying "You may lay it down, but you shall not use it except in a particular way," would amount to a prohibition.

Mr. Green.—Do you see any objection to compelling the company to allow persons to send any information they please by means of your telegraph? I see none at all, under particular arrangements, inasmuch as I think that is what they would do as a matter of course; but then it must be subject to certain regulations of the company; they could not consent to its being taken out of their hands, when they are using it, and given to another; of course the transmission of general intelligence would be one source of income derived from it.

Mr. Loch.—How would this operate upon the construction of a telegraph of this sort, if the government were to have the power, paying for it, to be enabled to lay down a telegraph of their own; would that operate with the directors in preventing their laying down one of their own? I think not at all; I cannot conceive that it would be their wish to prevent government possessing one. I think if an expenditure shall have been incurred by any company in laying down one under the expectation that they will derive the benefit of it, whether in transmitting railway information or general information, being properly paid for it, if they should be obliged to permit another company to lay down another telegraph on their line, that would be a great hardship; but I am sure they would do everything they could to facilitate the views of government.

Lord Granville Somerset.—Supposing the government were to lay down a magnetic telegraph from London to Bristol, and supposing that any parties were allowed, under certain regulations, to communicate by that telegraph, would you see any objection to that? I expressly reserved that it should be used for government purposes only. It never could answer to lay down two telegraphs; it would be a great hardship to make the possessor of the soil give up his right to enable some other party to compete with him.

Mr. Loch.—Confining it to government purposes, you see no objection to allowing the government to lay an electrical telegraph? None whatever; at the same time I should state that it is a subject I have not much thought of or considered, and therefore I fear my opinion its worth nothing.

Chairman.—Why was not your telegraph put under ground? It was attempted at first to be put under ground, but the wet got so much to it, it was found better to put it above ground, to secure it from that injury. I believe one of the great difficulties the patentee had to contend with at the time has been since remedied by making the tubes more impervious to the wet.—(Mr. Wheatstone.) I wish to make an observation with regard to the expense of the line: the cost of the present experiment has exceeded 250*l.* per mile. We will assume that it cannot safely be reduced, though I think with more experience it might be; if we consider that the cost of laying

down the whole telegraphic line from London to Bristol will be only the cost of one mile of the railroad itself, the expenditure will not appear great, considering the benefits to be obtained; this is less than one per cent. upon the original estimate of the expenditure. Now would it not be worth while to go to that expense to obtain all the advantages that will undoubtedly be obtained by the telegraph? I will make a few observations with regard to the proposed Government line. The principal expense of laying down the telegraph line is, in fact, the iron tube and the other things connected with it. The mere cost of the wires is very little, not more than 6*l.* or 7*l.* per mile each. As many wires may be put as you please in the same tube, consequently, supposing an iron tube to be laid down from hence to Portsmouth, if wires for three distinct lines were enclosed within it, the expense of each line, considered separately, would be very considerably diminished. One line might be appropriated for the rail-road purposes alone, another for general commercial intercourse, that is, for sending messages for any parties who choose to pay for the accommodation; and a third for the exclusive use of the Government. There would be no difficulty, if the Government have a telegraphic line thus associated with the others, to make the terminations in their own offices, from the admiralty in London, for instance, to any office belonging to the same department at Portsmouth, so that information may be sent without communicating with any persons but their own clerks. If this plan were adopted, it would do away with every objection which has been made with regard to the injury a private Company would do to the public, by having the exclusive means of intelligence in their own hands; and I am sure any railway company would enter willingly into an arrangement, by which Government might possess an exclusive line at a very moderate expense, much below that at which they could lay it down themselves. If the new telegraph of which I have spoken succeeds, and it has succeeded perfectly so far as experiments have yet been tried, we might place three telegraphs in connection with the six wires now used on the Great Western Railway, and these might be applied, as I have said before, to three specific purposes; one exclusively for railway purposes, another to be let to any persons who choose to avail themselves of it, and another for Government objects.

Would it be possible, by any portable instruments, for any third party to become acquainted with the messages sent on account of the government? If the government feared anything of that kind, they must use a cypher; communications by the electric telegraph would be far less public than by the present visual mode; at present every-body knows when a telegraph message is being despatched, and any person acquainted with the signals might read it.

Is it not the case, that by a little attention any person can possess themselves of any cypher? Very ingenious systems have certainly been decyphered, without any knowledge of their keys; but the task is no easy one. An extremely simple and safe mode of cypher

has been devised, by means of which a person may communicate with a thousand correspondents, it being impossible for any one of them to read what is intended for another.

Mr. Freshfield.—Have you made a calculation of the probable length of time the apparatus will continue, without requiring to be renewed? That is a question I cannot answer; but it comes to this: how long can the iron tube which contains the wires be preserved; the wires themselves would remain uninjured for an indefinite period, if the tube be kept perfectly water-tight.

Do you think the wear and tear of the apparatus from London to Bristol would be less than the wear and tear of the railroad for one mile? Far less.

Mr. Loch.—Do you spell every word by the present mode? Some signals are used, but the words of a message are generally spelt.—(Mr. Saunders.) We have some conventional signals; the others are spelt. While we were working the telegraph, we worked it for some time intermediately through the Hanwell station, to try the effect of dividing it into different lines of telegraph; there was evidently no perceptible difference of time from Drayton to Hanwell, and from Hanwell to Paddington; for the same party having a double instrument at Hanwell, the instant he saw the signals on one he touched the keys of the other; the effect is quite instantaneous; in that way it might be sent to almost any distance.

(A description of the dial-plate of this telegraph, and of the arrangement of the magnetic needles, and their helices, will be given in our next number.)—EDIT.

XLIX.—On *Electro-Magnetic Coil Machines*; BY THOMAS WRIGHT, ESQ.

DEAR SIR,—Having been lately making some experiments with a view of determining the most efficient form which can be given to the coil in electro-magnetic coil machines, and having succeeded in producing a machine of great power in proportion to the quantity of wire employed, I proceed to lay an account of my experiments before the readers of your valuable journal.

I have long considered that both the *intensity* and quantity effects of these machines are due, rather to the *intensity* than the *quantity* of magnetism developed in the central core of iron wires. The coils which I have seen produced by the London philosophical instrument makers have been invariably short and thick, a form which I think very ill adapted to the purpose for which they were intended, as by this means a great *quantity* of magnetism is produced, but possessing very little *intensity*.

During the course of experiments which I instituted I employed 11 coils, the structure of which was as follows:—

No. 1, consisted of a core of soft iron wires 1-40* of inch in diameter, and four inches long, wound with 40 yards of copper wire 1-16.

No. 2. Core one foot long, half an inch in diameter, iron wires 1-60; battery helix 40 yards 1-16; superimposed helix 60 yards 1-40.

No. 3. Core two feet by half an inch, iron wires 1-40, battery helix 60 yards 1-16, superimposed helix 60 yards 1-40.

No. 4. Core one foot long, one inch in diameter, iron wires 1-40, helix 40 yards 1-16.

No. 5. Core two feet by half an inch, battery helix 40 yards 1-10, superimposed coil 60 yards 1-16.

No. 6. Core one foot and a half by half an inch, helix twenty yards 1-10.

Nos. 7 and 8. Cores each eight inches long, by a quarter of an inch, iron wires 1-60, battery helices each 25 yards 1-16, superimposed helices each 50 yards 1-60.

No. 9. Core eight inches long by half an inch, iron wires 1-20, helix 25 yards 1-16.

No. 10. Core eight inches long, half an inch square, composed of seven strips of sheet iron carefully annealed, battery helix 30 yards 1-16, superimposed helix 100 yards 1-60.

No. 11. A compound U shaped bar 20 inches long, and two inches in diameter, composed of hoop iron riveted together and wound with eight copper wires 1-16, 20 yards long, all covered together so as to form a single helix; the whole apparatus being fitted up with a revolving armature for the purpose of breaking contact with the battery.

The effects of the above coils when connected with a seven inch pair of zinc and copper plates excited *a la Mullins* were as follows:

Shock.	Spark.	Sointillation, from iron or steel.	Number of seconds required to produce a measure of Gas.
1. Scarcely perceptible.	Small and bright	Very poor	48
2. From each helix strong, with both united would fasten with dry hands.	Large, bright, and snapping	Very good	28
3. With both helices united excessively strong	Much larger but less bright, having a flashing appearance similar to the firing of a grain of gunpowder	Very bright from steel, not so good from iron	43
4. Effects similar to No. 1	No. 1	not tried
5. Shock not so good as No. 2.	Very similar to No. 2	18
6. Shock just perceptible.	Large and bright	Very beautiful	10
7 & 8. Each of these coils with united helices gives a powerful shock; but when united in one length of 140 yards the shock is stronger than any I have yet experienced.	With both battery helices united in a double strand of 25 yards the spark is exceeding bright and large	brighter and larger than last	6
9. All the effects very	indifferent
10. Not perceptible with battery helix, would not fasten with both helices united.	Large and bright	Very good	22

* The diameter of the wires in fractional parts of an inch.

† When the helices are mentioned as united, it is meant that the end of the

11. The effects from this helix with the small battery above-mentioned were not so good as from Nos. 7 and 8 conjoined; when however it was connected with a series of four of Daniel's cells 3 feet high by 4 inches in diameter, the sparks and scintillations were very splendid; the decomposition with this battery were not tried, as I had not then a decomposing apparatus at hand.

All the iron used in the above coils was carefully annealed and the copper wire new and *had not been coiled at any previous time.*

From the foregoing experiments it would seem that the most efficient form that can be given to these machines, is that of an elongated helix enclosing a core of *thin* soft iron wires of not more than a quarter of an inch in diameter, and that if it should be required to increase the size of the machine, the number of coils and not their diameter or length, should be multiplied. The most advantageous length for the battery current seems to be about 20 or 25 yards, I am not, however, quite sure as to this, with regard to decompositions.

If the coils are multiplied great care must be taken that the length of wires, texture of metal, and method of coiling are *exactly similar* in each coil, otherwise the stronger currents will use the wire of the weaker ones as partial conductors, and thereby very greatly deteriorate the action of the whole of them: thus if we unite a helix of 40 yards and one of 60 in a double strand we shall not obtain near so strong a shock as from either of them singly; but if we unite two helices of equal length, the shock is sometimes better, the decomposition and deflagrations always so.

I have fitted up the coils 7 and 8 with my vibrating electrotome,* described in a late number of the "Annals": and by a particular arrangement of springs pressing against each other in various directions, I can instantly vary the quantity or intensity of the current, so as to obtain it from lengths of 25, 50, 100, or 150 yards, or from a double length of 25 yards.

The foot-board of the machine is hollow in order to admit a *flat* battery underneath.

Flat Battery.—Having noticed, (as I have no doubt many of your readers have) that in the ordinary cylindrical battery, the sal,

battery helix is joined to the commencement of the upper one so as to form one continuous coil, the battery current being passed through the thicker helix.

† With this coil and both helices united, I was enabled to imitate the action of the *Gymnotus Electricus* with great success, by inserting two pieces of tinfoil in a tub of water and connecting them with the coil; if the hand was placed in the water a shock was immediately felt which was very severe when both hands were immersed.

* In my published description of this electrotome I was sorry to perceive an error in the drawing; the contact was there shown to be broken at the end of the spring, whereas it ought to have been at a point about an inch and a half nearer the middle of it; an electrotome thus constructed will vibrate a little time after being jerked by the finger, without the aid of the coil: it will be advisable to have the touching points, *which should be small*, tipped with platina; the spring should be about four inches long and press strongly on the brass bar against which it vibrates; some of the electrotomes that I have thus made are *quite musical*.

in the cupreous solution is very liable to subside, by means of which, the action, when long continued, is in a great measure confined to the lower part of the arrangement, which is evident from the copper becoming considerably thicker at the lower, than at the upper part, I was led to construct a battery in the following manner:—a piece of thin sheet copper was bent up at the edges in the form of a tray, in the inside of this was placed a similar tray of thin mil-board, cemented at the sides, and furnished with small legs of sealing wax.—Zinc and salt and water in the mil-board tray—sulphate of copper in the copper one.

This battery has many advantages:—it is constructed at an expense not greater than the cost of the materials—the action is very equable—and it can be easily slipped under the foot-board of any piece of apparatus to which it may be applied; if it is required to employ a series of them, they may be piled upon each other, the wires of the zinc plates pressing with a spring on the copper next above; and as it is not necessary that the trays should be more than an inch deep, a large battery on this construction might be packed in a comparatively small space.

Mr. Uriah Clarke has, I am sorry to observe, accused me of copying his electro-magnetic machine; this is certainly not the case, and if necessary I could adduce proof to the contrary; I cannot say however that I see much resemblance in the two arrangements further than that they have each a reciprocating motion; I consider Mr. Clarke's much superior to mine as to the crank point, though I think he will find that great power is gained by partially continuing the armatures on the magnet. I have not proceeded with my engine, as I fancy I have hit upon a more efficient plan. I have very little expectation however of these engines being applicable to the working of machinery;—at least economically. They are very interesting toys.

I am, my dear Sir,

Very truly your's,

William Sturgeon, Esq.

THOMAS WRIGHT.

L.—*On the Theory of Ætherfication, (Ætherbildung);* By PROFESSOR HEINRICH ROSE.

It is known that many salts of the oxide of bismuth, of the oxide of quicksilver, of the oxide of antimony, and other metallic oxides, become decomposed by water. They, usually, by that means become transformed into basic salts: but sometimes by the application of a sufficient quantity of water, the decomposition proceeds to the separation of pure oxide; as, for instance, with the nitrate of the oxide of quicksilver.

The explanations which are usually given of these decompositions

is, that we admit that the water resolves the neutral salt of a metallic oxide into an acid and a basic salt, in a similar manner to that in which nitric acid transforms red super-oxide of lead into protoxide of lead, and brown super-oxide of lead. But the existence of acid salts, which, by the influence of the water on several neutral salts of metallic oxides, as has been supposed, is not satisfactorily proved: for in most cases the water takes only a part of the acid from the salt, and this dissolves some of the neutral salt, which, near the point of concentration of the acid solution by evaporation, in most cases crystallizes and separates as a neutral salt; and but very seldom as a double combination of neutral salt and acid hydrate. In many cases the quantity of salt which dissolves in the acid is exceedingly small; sometimes not any, and the whole quantity of the oxide forms an insoluble basic salt.

The easiest explanation which we can give of these decompositions by water, appears to me to be this, that it is the water which acts as a base, and separates the oxide as a basic salt; or, sometimes even in a pure condition, and combines with the acid to form a hydrate. This explanation will become the more admissible as we have already been accustomed to consider the hydrates of acids as saline compounds, in which the water represents a fixed base. It is well known what fertile inferences for the whole theory of chemistry have been drawn by these views of the nature of the action, and especially by Graham, Berzelius, and Liebig.

In fact, it is particularly the salts of such metallic oxides as are not possessed of strong basic properties which by water become decomposed. The salts of the more formidable bases do not display this phenomenon.

According to this view these decompositions are analagous to the conversion of the red oxide of lead into the brown super-oxide and the protoxide of lead by means of nitric acid, only that they are of a directly opposite kind; for the strong acid, in a combination of protoxide of lead with the peroxide, expels the electro-negative body, and combines with the basic.

The water occurs, also, as a base in other cases, and sometimes displaces other bases from their combinations. As, however, it always belongs to the weaker bases, and is at the same time volatile, such cases are not very frequent. But although itself volatile, it can expel the volatile oxide of ammonium from its combinations. If a solution of sulphate of the oxide of ammonium be boiled for a long time it becomes acid, and provided the boiling be in a retort, a fluid, containing free ammonia, distils over into the recipient. This result obviously proceeds from the water, as a base, expelling the oxide of ammonium, (which in a *free state* cannot exist, but is resolved into ammonia and water) from its combination with sulphuric acid, with which it enters into a combination. The quantity of the sulphate of the oxide of ammonia, which, in this way, becomes decomposed, is certainly very small; we must, however, consider that the oxide of ammonium belongs to the most powerful bases, and this result is principally to be attributed to its superior volatility.

If we apply the foregoing explanation of the decomposition of many salts by water, to the theory of Ætherification, much simplicity will be derived.

Berzelius and Liebig have adopted the view, that the æther may be regarded as a base, which view has found such general approbation that it is become almost universally adopted, at least in Germany.

It is known that the salts of the oxide of æthyl, (the compound æthers) by bases, become more or less easily decomposed when water is present: for those bases associate themselves with the acid of the compound, and liberate the oxide of æthyl as a hydrate (alcohol.)

The same decomposition, nevertheless, is also caused by water, which, in this case, obviously operates as a base. Some compounds of the oxide of æthyl become as easily decomposed by water, as by the operation of many other bases; as, for instance, is the case with oxalic æther, which, by water, becomes resolved into hydrate of oxalic acid and alcohol. To accomplish this transformation it is not necessary to employ a high temperature, because it takes place even at the common temperature, and, indeed, in a very short time.

The acid sulphate of oxide of æthyl, or rather the combination of the sulphates of the oxide of æthyl with hydrated sulphuric acid, (sulphovinic acid) in its solution in water, also suffers a precisely similar decomposition. Even at the common temperature, in this case, will alcohol and hydrate of sulphuric acid be gradually formed: but their formation is much quickened by boiling.

This process may also be easily explained on the supposition that the water operating as a base liberates the oxide of æthyl from its combination with the sulphuric acid, which, at the moment of separation, takes up water and forms alcohol.

The aqueous solutions of nearly all the sulphovinates become similarly decomposed, and especially when boiled. Alcohol and water evaporate, and in the solution is formed a so called acid sulphate, that is to say, a double compound of the neutral salt, which, with the hydrate of sulphuric acid, pre-existed in the sulphovinate salt.

If sulphovinic acid be heated with only a small quantity of water, no alcohol will be obtained, but principally hydrated sulphuric acid, and pure oxide of æthyl, or æther. There is not a sufficiency of water present to transform the liberated æther into alcohol.

If alcohol be mixed with hydrated sulphuric acid, sulphovinic acid becomes formed, or a double compound of neutral sulphate of oxide of æthyl with the hydrated sulphuric acid. By the formation of sulphate of oxide of æthyl two atoms of water are set free; one from the hydrated sulphuric acid and the other from the alcohol. By heating the mixture, one of these free atoms of water liberates oxide of æthyl from its combination with sulphuric acid, combines with the acid, and forms hydrated sulphuric acid.

But why does not the æther combine with water at the moment

it is liberated, and thus form alcohol? The water is sufficiently plentiful, because the liberation of the æther requires only one atom of water; and at the formation of sulphovinic acid, even when anhydrous alcohol is employed, two atoms are set free.

It is known, that sulphuric acid can take up more than one atom of water to form a hydrate. We know, also, that, besides the common hydrate with one atom of water, there is a second, which can be prepared in a crystalline state, and which contains two atoms of water. This compound corresponds to a basic sulphate salt.

The disposition of the hydrate of sulphuric acid to take up more water is very great, and it is employed on this account for various purposes in our laboratories. It is this which prevents the æther, originating from the decomposition of the sulphovinic acid, from taking up the second atom of water; but if the mixture is uninterruptedly boiled for some time, the hydrated sulphuric acid loses the acquired water, which may then be distilled over in company with the æther. The æther may therefore, from a boiling mixture of the hydrate of sulphuric acid and alcohol, be distilled over at the same time with water; but they are not the products of one, but of two chemical processes, which are both active together in the boiling mixture.

At the commencement of the operation, but very little water passes over along with the æther and that alcohol contained in the mixture, which has not been converted into sulphovinic acid so, that the water remains dissolved in the distilled alcoholic æther, and does not separate: the quantity of water increases by further distillation, especially at a high temperature, when the quantity of the second hydrate of sulphuric acid has augmented.

Anhydrous alcohol is scarcely ever employed in the preparation of æther, but generally hydrated. It is evident that in the latter case the quantity of the second hydrate of sulphuric acid must be considerably increased. The experiments of Liebig, Magnus, and Marchand have shown that in the cold this second hydrate cannot form sulphovinic acid with alcohol, but does so at a higher temperature, and therefore that such a mixture on boiling can give æther by distillation. But it is known that by employing hydrated, or even anhydrous alcohol, there is always a portion of it which is not converted into sulphovinic acid, and this quantity may be distilled as alcohol from the mixture. A second portion of alcohol, which distils over in company with the æther, in the formation of æther, may, however, be produced in this way,—that æther and water are contemporaneously disengaged from the mixture, and combine to form alcohol; for it is produced only in this way when a solution of pure sulphovinic acid is boiled with much water, or compound æthers decomposed by water or by the hydrates of bases.

When, however, from the tendency of the hydrate of sulphuric acid to take up more water, æther has been evolved from a mixture of alcohol and sulphuric acid, it does not take up any water after being once separated: but water may be distilled over by heating

the diluted sulphuric acid. We know that when æther is treated with water, or even dissolved in it, no alcohol is formed. When æther is once separated from a compound of oxide of æthyl, the former can in no way be converted by water into alcohol. Only when, as above observed, the æther comes in contact with water at the moment of its expulsion does it form alcohol with it. The contemporaneous disengagement of æther and water, from a boiling mixture of alcohol and the hydrate of sulphuric acid, shows therefore quite evidently that both owe their origin to two distinct processes.

Moreover, it is by no means an anomalous phenomenon that a base, which is capable of forming a hydrate, does not combine with water when brought into contact with it in a pure state; a great number of cases of this kind occur in inorganic chemistry. We need only compare æther with that numerous class of ignited oxides in which so compact a state of cohesion is produced by heat, that they not only withstand the action of water, but even entirely or partially that of acids, to find abundant proof of such analogies. The ignited oxides with these properties always belong to the weaker bases, under which æther must necessarily be classed. Æther may be assimilated to these oxides the more, as it like them combines directly with acids with difficulty.

But even among the stronger bases we find some whose relations to water resemble those of æther. When oxide of copper is precipitated in the cold by bases from solutions of salts of the oxides of copper, it appears as a hydrate of the oxide of copper; which, however, on being heated under water, loses its water, and does not take it up again when left in contact with it at a higher, or at the common temperature.

To find out at what period, in the preparation of æther by boiling a mixture of alcohol and sulphuric acid, water commences to pass over, M. Wittstock, at my request, instituted a series of experiments, which he had the kindness to communicate to me.

Two pounds of the hydrate of sulphuric acid were mixed cold with two pounds of anhydrous alcohol, the mixture was made to boil with all possible haste in a retort, the distilled products, well cooled, were gradually received, and the distillation continued until the contents of the retort boiled over.

The weight and specific gravity of the products were determined as they distilled over in succession. The results are as follows:—

First distillation: 3 drachms 50 grains; spec. gr. 0.776*; produced before the boiling of the mixture. The following products passed over after its boiling:—

Second: 3 ounces 6 drachms; spec. gr. 0.808.

Third: 3 ounces 6 drachms; spec. gr. 0.800.

Fourth: 3 ounces 6 drachms; spec. gr. 0.786.

Fifth: 3 ounces 5 drachms 50 grs.; spec. gr. 0.776.

Sixth: 4 ounces 1 drachm 50 grs.; spec. gr. 0.761.

* The specific gravities, both here as well as those to be mentioned subsequently, were all determined at 14° Reaum. (63.5° Fahr.).

Seventh: 1 ounce 7 drachms 10 grs. ; spec. gr. 0·809.
Eighth: 1 ounce 2 drachms.

The first five products consisted of a single liquid ; the sixth was the first in which a layer of water and of æther were perceptible. The quantity of separated water amounted to 3 drachms ; the æthereal liquid had the specific gravity mentioned above. The seventh product consisted in volume of two parts water, and three parts of an æthereal fluid of the specific gravity stated ; the eighth consisted almost entirely of water, above which floated a very thin layer of æther, which was coloured yellow by oil of wine. The contents of the retort boiled over on the continued application of heat.

The first five products consisted of æther mixed with alcohol, which last was contained in the retort as such, and not converted into sulphovinic acid, and evaporated from the mixture in company with the æther. The first product, which distilled over at the lowest temperature, contained, to judge from its specific gravity, much æther, and little alcohol, quite opposed to the general opinion that æther is only formed at the boiling-point of the mixture. The succeeding products gradually became, according to their specific gravity, constantly more æthereal, and contained less alcohol ; but only in the sixth product was there so much water that it separated, and the quantity increased in proportion as the distillation was continued.

The first six products smelt but slightly of oil of wine ; but the seventh contained a portion, and also smelt of sulphurous acid. After the first seven products had been mixed together, and the separated water removed, they had a specific gravity of 0·788.

It is well known that æther is prepared, of late, in the most advantageous manner, by allowing a small stream of alcohol to flow constantly into a mixture of alcohol and the hydrate of sulphuric acid. It has been denied that the presence of sulphovinic acid is of essential influence in the formation of æther, and asserted that it is not necessary that the formation of this acid should precede that of æther, because in the method of preparing æther alluded to, the boiling mixture must be constantly at a temperature of 140° cent., at which sulphovinic acid could not exist. But at the point where the current of cold alcohol flows into the boiling mixture, the temperature is under 140° C. The sulphovinic acid formed is decomposed it is true, in a very short time, from its soon acquiring the temperature of the boiling liquid. The preparation of æther, according to the above method, consists therefore in a constant formation, and continual decomposition of sulphovinic acid. It is a pretty generally entertained opinion that the production of æther from a mixture of alcohol and sulphuric acid, is solely effected by the boiling of the mixture, which takes place at a high temperature, about 140° C. In many works on chemistry we meet with the assertion that when a mixture of sulphuric acid and alcohol are heated

at a temperature, not high enough for it to boil, no æther, but merely anhydrous alcohol, is obtained.

Were this assertion correct, it would be an important objection to the hypothesis I have advanced; for, according to that, it would be somewhat difficult to explain the circumstance why the oxide of æthyl is separated at a lower temperature, as a hydrate, and at a higher one in an anhydrous state.

But this common opinion is founded on an error, which to me is quite incomprehensible. Æther is obtained even from a mixture of the hydrate of sulphuric acid and anhydrous alcohol, when distilled in a water-bath, at a temperature which need not always amount to the boiling heat of water. It is not indeed requisite to employ anhydrous alcohol, but the hydrated, of 90 per cent. Tralles*, to obtain æther from a mixture at the above-mentioned temperature.

Mr. Wittstock, at my request, had the goodness to institute a series of experiments on this point, and communicated the result to me.

I. Fifteen ounces of anhydrous alcohol were mixed in the cold, with an equal weight of the hydrate of sulphuric acid, and the mixture distilled at a temperature at which it could not boil strongly. The products, well cooled, were successively received, and the temperature at which they passed over accurately noted.

<i>First product</i> : 1 dr. 10 grs., spec. gr. 0.817,	
passed over at from	60° to 80° R.
<i>Second product</i> : 3 oz. 1 dr. 10 gr., spec. gr.	
0.792, passed over at from.....	90° —93° „
<i>Third product</i> : 3 drs. 57 grs., spec. gr. 0.772,	
passed over at from	75° —80° „
<i>Fourth product</i> : 2 oz. 40 grs., spec. gr. 0.749,	
passed over at from	90° —95° „
<i>Fifth product</i> : 5 drs.	

When the mixture had reached the temperature of 90° it began to boil very slightly; the boiling, however, subsequently ceased at this temperature, but even then æther was disengaged from the mixture in bubbles, just as carbonic acid gas escapes at the common temperature from a liquid strongly saturated with it.

From these experiments it is evident that æther is formed at far lower temperatures than is usually supposed. The first product smelled indeed strongly of æther; but chiefly consisted, which is also indicated by the specific gravity, of alcohol, which had not been converted, by mixing with sulphuric acid, into sulphovinic acid; æther could not be separated from it, either by water or even by chloride of calcium. The second, third, and fourth products consisted, on the contrary, principally of æther, which could even be separated by mere washing with water. The fifth was the first that contained free water, and indeed, in volume, more than the half. The specific gravity of the æthereal liquid floating above it

* That is 90 per cent. absolute alcohol by volume.

was not determined. This last product distilled over very slowly, although at times the temperature was raised to 100° R.

It results from these experiments that æther which is produced at lower temperatures than is requisite to boil the mixture, is at the same time purer, and contains less alcohol and water than æther which has been prepared by strong boiling. A comparison of the specific gravities with those previously mentioned, set this evidently beyond all doubt. At a low temperature the water especially escapes later, and therefore only in the last product could separated water be observed, a proof that it is not disengaged in company with the æther.

II. A second series of experiments proved this in a still more decided manner, so that there can no longer remain any doubt on the subject that æther can be evolved in abundance at the boiling-point of water.

Seventeen ounces of anhydrous alcohol of specific gravity 0.792 were mixed cold with 18 ounces of the hydrate of sulphuric acid, and the mixture subjected to distillation in a water-bath whose temperature frequently did not even attain that of boiling water. The quantities taken are in the proportion of single equivalents of each of the substances employed; they were taken in this proportion, partly because it approaches that which otherwise is employed in the preparation of æther; when equal parts by weight of alcohol and sulphuric acid are employed, and also in order to have no excess of sulphuric acid.

The results of the experiments are as follows:—

First product. 3 drachms.

Second product: 3 ounces 6 drachms; spec. gr. 0.755.

Third product: 3 drachms; spec. gr. 0.745.

Fourth product.

Even the first product consisted of nearly pure æther; for a solution of acetate of potash separated æther from the liquid to the amount of two thirds of its volume.

The fourth and last products contained free water, and consisted of nearly half of it by volume; but it distilled over so slowly in the water-bath, that several hours were necessary to obtain a few drachms of it. From the specific gravities it will be perceived that the second, and especially the third product, consisted of æther far more pure than is obtained in other modes of preparing that substance.

III. As the idea is so general, that æther is formed from a mixture of alcohol and sulphuric acid only on boiling, and as in the usual mode of distilling, hydrated, and not anhydrous alcohol is employed, a new series of experiments were performed with the former.

A pound of alcohol of 90° Tralles, such as is usually employed in the preparation of æther, was mixed in the cold with a pound of the hydrate of sulphuric acid, and the mixture subjected to distillation in a water-bath, as in the second series of experiments. The results were:

First product : 4 drs. 36 grs. ; spec. gr. 0.833.

Second product : 2 oz. 4 drs. 20 grs. ; spec. gr. 0.787.

Third product : 4 drs. 50 grs. ; spec. gr. 0.789.

Fourth product : 5 drs. 17 grs. ; spec. gr. 0.789.

Fifth product.

The first product consisted almost entirely of alcohol, as indicated by the specific gravity. The succeeding ones contained much æther, or consisted mostly of it. Free water also was evident in this case only in the fifth and last product, which consisted of one drachm of liquid, of which only one fourth was separated water. To distil this small quantity over, it was necessary to heat for more than five hours.

The æther obtained from a mixture of sulphuric acid and alcohol, at the temperature of boiling water, is far more pure, as may be anticipated, and is indicated by the specific gravities of the products, when anhydrous, instead of hydrated, alcohol is employed. The æther obtained from hydrated alcohol in this way contains more alcohol, because upon mixing hydrated alcohol with sulphuric acid, less is converted into sulphovinic acid, and more remains in a free state in the mixture, than when absolute alcohol is used. According, however, to the theory advanced in this memoir, only that portion of the alcohol can produce æther which has been converted into sulphovinic acid, and this æther distils over when heated, in company with the free alcohol.

The fact that æther is produced from a mixture of alcohol and sulphuric acid even at the boiling-point of water, is indeed highly important in the theory of the formation of æther, and by this method the æther is also obtained more pure, especially from water, and of a far lower specific gravity than when distilled at a boiling heat ; but it is not convenient in the preparation of æther, in so far as at this low temperature the æther, and particularly the last products, pass over with great slowness.

One fact, however, seems not to admit of being quite satisfactorily explained by the present theory. Seeing that water acts as a base upon the oxide of æthyl, and disengages it from its combinations, it must appear surprising that stronger bases than water do not effect this separation still more perfectly. But solutions of the sulphovinate of potash and soda may be treated with an excess of potash without the oxide of æthyl being expelled ; and even the salts of the alkaline earths can exist in contact with an excess of base.

But there seems to be a difference in properties between the double compound of the hydrate of sulphuric acid with the sulphate of the oxide of æthyl and the other sulphovinates. The former is far easier decomposed by water than the latter ; but this fact is by no means without analogy. Water is able to decompose many salts of the oxide of antimony, and displace the latter from these combinations as a basic salt ; but the combinations of the oxide of antimony with tartaric acid, and other unvolatile organic acids, are not decomposed by water.

According to the earlier method in use, æther was obtained from a mixture of equal parts, by weight, of sulphuric acid and alcohol; but there is more alcohol at the commencement than is requisite. In the progress of the distillation, however, the quantity of sulphuric acid becomes constantly predominating, in proportion as the alcohol passes over as æther; and from the great excess of the hydrate of sulphuric acid, the liberated æther is itself decomposed by the boiling, which in this case takes place at a high temperature, and is then first converted into a double compound of the sulphate of the oxide of æthyl with sulphate of ætherol (Weinöl.); and lastly changed by the boiling into olefiant gas, from the presence of too great a quantity of the hydrate of sulphuric acid, and from too high a temperature.

This change of æther into oil of wine and olefiant gas, by an excess of sulphuric acid and too high a temperature, is not the result of a mere deprivation of water, as might be concluded from a comparison of the composition of these substances with that of æther; for as soon as the slightest trace of oil of wine is evident in the formation of the æther, a corresponding trace of sulphurous acid is disengaged, the quantity of which becomes more considerable if olefiant gas is formed. The production of sulphurous acid stands therefore in definite connexion with that of the oil of wine and olefiant gas. Since the origin of these two bodies takes place only at a high temperature, especially that of the olefiant gas, these substances undoubtedly owe their origin to a similar action of sulphuric acid on æther, as this acid exerts on other bodies of organic origin at high temperatures. The sulphuric acid is coloured black by these, at the high temperature, with the evolution of sulphurous acid and separation of a carbonaceous substance; the same also takes place in the distillation of æther, when continued to the production of oil of wine and olefiant gas.

The origin of this coal matter, which has recently been examined by Erdmann and Lose*, stands therefore in connexion with that of the sulphurous acid, oil of wine, and olefiant gas; consequently the formation of this body is the result of another process, which very likely has nothing to do with the formation of the æther.

When therefore æther is prepared from a mixture of sulphuric acid and alcohol at a very low temperature, it is perfectly free from oil of wine; and, in fact, not a trace of that substance could be observed in the first products which were obtained by the above distillations, not only in those that were performed in the water-bath, but also in those which were carried on at a gentle heat in the sand-bath. Even the last products appeared to be perfectly free from it; but if a considerable quantity of the æthereal liquid was evaporated on blotting paper, a very slight smell of it might be discovered, a trace however so insignificant, that individuals not well acquainted with the odour of oil of wine could not perceive it. Moreover, when the distillation was at an end, the residuum

* Poggendorff's *Annalen*, vol. xlvii. p. 619.

in the retort was, it is true, of a dark colour, but not deep so that it resembled a brownish vitriol, such as frequently occurs in commerce; the residue smelt as slightly of sulphurous acid as the distilled æther did of oil of wine. Not a trace of carbonaceous substance was separated. The process by which oil of wine is produced, commences, therefore, in the mixture prepared for the distillation of æther, even at the boiling-point of water, at least when this is long continued; but even then the formation of this body at that temperature is quite trifling in amount.

When æther is distilled from a mixture of sulphuric acid and alcohol in the water-bath, we obtain, as is evident from the above results, less æther than we might expect from the quantity of alcohol employed, and the residue weighs more in proportion. In the last series of experiments described, in which æther was prepared in the water-bath, the residuum, on employing 17 ounces of absolute alcohol and 18 ounces of sulphuric acid, weighed 27 ounces, and the distilled alcohol æther $4\frac{1}{2}$ ounces; the loss consisted partly in the water distilled, the quantity of which was not determined, in volatilized æther, which in this case volatilized the more, as it was nearly pure, and also in the loss which occurs by pouring out. On employing one pound of hydrated alcohol and one pound of sulphuric acid, the residuum weighed $26\frac{1}{2}$ ounces, the products 4 ounces and some drachms; the loss consisted partly in the water which passed over, the quantity of which was not accurately determined. In both cases therefore, besides water, æther also remained with the sulphuric acid, undoubtedly as isæthionic acid, probably also in part as æthionic acid. It is very probable that the products which present themselves with æther in a distillation when long continued and at high temperature, are produced, not by the direct decomposition of the æther, but by the decomposition of the isæthionic acid, occasioned by the excess of sulphuric acid and a high temperature; such as the precipitated carbonaceous substance, the sulphurous acid, oil of wine, and lastly, the olefiant gas.

It is well known that the formation of these products is generally avoided in the preparation of æther by the new and most profitable method, in which, as æther passes over, a like quantity of alcohol is allowed to flow into the boiling mixture. The action of an excess of sulphuric acid on the alcohol, or rather on the isæthionic acid, at a high temperature, is thus prevented.

When formerly the production of æther was sought to be explained by the subtraction of the water from it, by means of sulphuric acid, it might with much justice be objected to the present explanation, that other bodies, which have, like sulphuric acid, a great affinity to water, such as the hydrate of potash, chloride of calcium, &c., are not able to transform alcohol into æther; but this objection now falls entirely to the ground, as we know that the æther is not formed by any subtraction of water, but by the decomposition of the sulphovinic acid.

If æther is regarded as a base, then all the theories on the formation of æther are not capable of satisfactorily explaining how a base

is discharged from a strongly acid liquor, and by a powerful acid. It is only by the present explanation, and by the analogy which the separation of æther from sulphovinic acid bears to the decomposition of several inorganic salts by means of water, and also by the above-mentioned analogy of æther with a series of oxides which do not, or to a very slight extent, combine with acids, that this phenomenon loses its anomalous appearance.

It seems to me highly desirable in organic chemistry, to illustrate its processes always as much as possible by analogous processes in inorganic chemistry. The greatest advantages have accrued to organic chemistry by the endeavours of Berzelius, Liebig, and Dumas, who have pursued this path, frequently starting, it is true, from very different views.

It is certainly advantageous in so imperfect a science as chemistry, and especially organic chemistry, to ascribe provisionally to a common force all phenomena which stand isolated, for which no suitable analogies can be detected, and which on this account appear wonderful, and thus openly to admit that in the present state of science it is better to avoid explaining a process altogether, than to explain it by some artifice or in a constrained manner. The smaller the number of phenomena which we are compelled to refer to this class, the more perfect the science becomes.

Setting out from this point of view, I have ventured to explain a process in organic chemistry, which has long, and particularly of late years, engaged the attention of chemists, as being analogous to several processes in inorganic chemistry; and if the explanation should not give general satisfaction, the attempt to attain so important an object, will, I trust, meet with approbation.

The present theory is valid, it is true, only for the formation of æther from a mixture of alcohol and sulphuric acid; but quite a similar one may undoubtedly be advanced for the formation of æther from mixtures of phosphoric and arsenic acids with alcohol. For the present, however, I leave it undecided whether the formation of æther, by treating alcohol with fluoboracic gas, as also with the chloride of zinc and other chlorides, is to be explained by a mere subtraction of water by these substances; or in this way, that they form with alcohol, at the common temperature, combinations analogous to sulphovinic acid, which are decomposed like it, at a high temperature, by the agency of water. The latter view I regard as being the most probable.

POSTSCRIPT.

In the preceding Memoir I have compared the formation of æther from a mixture of sulphuric acid and alcohol, with the decomposition of several inorganic salts by means of water; I have endeavoured to show that it is the water which in these cases acts the part of a base, and separates the oxide of æthyl or the metallic oxide, the latter generally as basic salt.

The inorganic salts which I enumerated in this comparison as examples, were those of the oxide of bismuth, the oxide of mercury, and of antimony. These undergo the said decomposition by water even at the common temperature; æther, however, is first separated from a mixture of sulphuric acid and alcohol, or from sulphovinic acid, at a high temperature.

There are, however, among the inorganic weak bases, a considerable number which are eliminated by water, from their combinations with acids only at a high temperature; and the decomposition of the salts of these bases, by means of water, is therefore still more fit to be compared to the formation of æther.

To these bases belongs more especially the peroxide of iron, which is precipitated by water as basic salt from solutions of most of its neutral salts at a high temperature. The weaker the solution of the salt of peroxide of iron, the lower is the temperature which occasions precipitation, and the more completely is the peroxide of iron thrown down, so that with a certain dilution as M. Scheerer has shown, scarcely a trace of the peroxide of iron remains in solution, but the entire quantity is separated as basic salt. As stronger bases are not precipitated by water on boiling, this property of the peroxide of iron has been employed to separate it from the oxides of cobalt, nickel, and other metals†. It may even be separated, by boiling the solution, from alumina, which, although it has with regard to its properties much similarity to the peroxide of iron, is evidently a stronger base; this separation of alumina from the peroxide of iron by means of water at a high temperature, is of some importance to the arts, as in the fabrication of alum the peroxide of iron contained in the mother-liquor is precipitated by mere boiling, and is thus more easy to separate from the alumina than the protoxide of iron, although the former, with sulphuric acid, and an alkali, forms an alum which has quite an analogous composition with alumina-alum; and, from being isomorphous with that alum, could crystallize with it in all proportions.

Several other bases have the same property as the peroxide of iron, which like it belong to the class of weaker bases, and also several substances which act as bases towards strong acids, and also as acids towards strong bases, and which on that account are frequently classed among the acids. Among these are the oxide of zirconium, thorina, the peroxide of cerium, peroxide of tin, titanous acid, tellurous acid, columbic acid; also in certain respects molybdic acid, tungstic acid, and vanadic acid. Several combinations of these oxides with acids are soluble in the cold in water, and are precipitated from the solution, on boiling, as oxides or basic salts.

Several of the oxides precipitated in this manner possess, after precipitation by boiling, properties which they do not evince before their solution in acids and precipitation; they are more indifferent than before, are partly of difficult solution in acids, partly insoluble, and do not combine after precipitation with them, even when these are employed in a concentrated state. Titanous acid, peroxide of tin, and many others may be classed here. This peculiarity is

in a certain degree analogous to that of æther, which, when it has been once separated by boiling from a mixture containing sulphovonic acid, appears not to combine directly with acids.

LI.—On Galvanic results, in letters addressed to Prof. Silliman, October 4, 1838, and August 6, 1839, from the vicinity of London; by WILLIAM STURGEON, Esq. •

REMARKS BY THE EDITORS.

A very economical and efficient voltaic arrangement was adopted by several members of the London Electrical Society, and the report of the construction and performance of the battery, in a series of experiments performed at Clapham Common in the autumn of 1838, is contained in the report of Mr. Charles V. Walker, published in the Transactions of the London Electrical Society, in two papers dated October 16, and November 6, 1838. In allusion to this battery, Mr. Sturgeon observes, in his letter of October 9, 1838:—

“A voltaic battery has been got up (at the expense of two of our leading men, whose names I am not at liberty to mention,) for the sole purpose of investigation. The battery consists of one hundred and sixty porcelain pint jars, each containing a copper and zinc cylinder; the latter being covered with stout brown paper, is introduced to the interior of the copper. The exciting fluids are solutions of sulphate of copper and muriate of soda; the former applied to the copper cylinders, and the latter to the zinc ones. When the jars were in series the flame was upwards of an inch long, from a charcoal point, rotated on the poles of a magnet, according to the principles of electro-magnetism. Davy deflected the electrical flame by magnetic influence, but I am not aware that he rotated it.”

“Sulphuret of lead (galena) was decomposed, and metallic lead obtained. Sulphuret of antimony was decomposed, and the liberated metal kept in fusion for several minutes. The boiling antimony was *three inches long* and half an inch wide between the polar wires, and exhibited a beautiful spectacle, in a channel of those dimensions which the action had formed in the native sulphuret. When the electric flame was directed through the air between stout copper polar wires, the positive wire became red hot, but the negative wire could not be made red. The wires were made to change poles, still the same thing occurred: nay, even two inches of the positive wire, which was completely out of the circuit, was rendered hot, but no redness appeared on the negative wire. How exceedingly curious and interesting is this last result!

“When the whole battery was formed into eight groups of twenty jars each, and properly connected with an electro-gasometer, the mixed gases were liberated from water at the rate of one cubic inch per seven seconds: and this for many successive minutes, although the battery had been in action for seven previous hours without interruption.”

In his letter of August 6, 1839, Mr. Sturgeon proceeds to observe, that a good description of the apparatus and experiments will be found in the memoir above named, and of which he kindly transmitted a copy. But he remarks: "there are some particulars connected with the discovery of the *difference of temperature*, produced in the positive and negative wires, which want a clearer description than any given by Mr. Walker, or, perhaps any which that gentleman had then a means of giving; and, as I find, from the defective information which has been given of this particular discovery on the continent of Europe, that M. De la Rive and others, have failed in reproducing the curious phenomenon, it is possible that the American philosophers may also fail from a like cause, were the particulars of manipulation not made known to them. I will, therefore, for the information of all the readers of your excellent journal, give a brief historical sketch of the whole business."

"The battery consisted of a hundred and sixty white porcelain jars, each of the capacity of about two thirds of a pint, and furnished with a hollow cylinder of sheet copper, and an interior hollow cylinder of sheet zinc, the latter amalgamated, and in metallic connexion with the copper of the next pot, &c. The copper and zinc of each pot were separated from each other by a diaphragm of brown paper, (a disc, on the centre of which is placed the centre of the base of the zinc cylinder, and the periphery brought up to the upper end of the latter so as to form a bag round the zinc,) which separates the solution of sulphate of copper, which is placed *outside*, from the solution of common salt, which is placed *inside* of it. Hence the copper is washed with its sulphate solution, and the zinc with the muriate of soda solution.

"One hundred of these metals and pots were furnished by Mr. Gassiot, and the other sixty by Mr. Mason. The preparation of a battery of this kind and extent is a great labour, as you will understand from the following particulars. Mr. Walker commenced working at it between eight and nine in the morning; Mr. Mason arrived about eleven in the forenoon, and immediately set to work at it; Mr. Gassiot commenced shortly afterwards, and it was not ready for experiment till three in the afternoon, about an hour and a half after I arrived at Mr. Gassiot's house. The plan of dividing the battery into groups for the experiments on decompositions was formed by Mr. Mason, who is a very skilful and neat experimenter.

"At a previous meeting I was requested to provide a catalogue of experiments, which I did; but in consequence of the great length of time occupied in the experiments on the decomposition of water by the various forms of the battery, only a few of them were attempted. As the decompositions are very well described by Mr. Walker, it would be unnecessary to say anything more about them in this place. They were carried on with great exactness in the following manner. The graduated glass tube of the electro-gasometer being filled with acidulated water and inverted over the platinum terminals of the instrument, one of the polar wires of the battery was connected with it, and the other kept in the hand of the experimenter ready to plunge into the other mercurial cup of the instrument the moment the word "time" was given, and taken out again when a cubic inch of the gases was collected.

"With regard to the experiment in which I discovered the great

difference produced in the two polar wires, it was undertaken from the views which I had long entertained concerning the non-identity of the *electric* and *calorific* matter, as you will see I have hinted at, at the close of section 1, of my first memoir to the London Electrical Society. It was late in the evening before I had any opportunity of making the experiment. The rest of the party were engaged in something else at the time, and the battery was in series of one hundred and sixty pairs. I brought the tip ends of the polar wires (copper wire one tenth of an inch diameter) into contact, end to end, then withdrew them gently and very gradually from each other, keeping the flame in full play between them till they were separated about one-fourth of an inch. In a few minutes the positive wire got red hot for half an inch, but the negative wire never became red. I repeated this several times, in order to be convinced of the fact. I next laid the wires across one another, and brought them into contact about an inch from the extremities, and separated them as before. In a short time the whole of that part of the positive wire from the point of crossing to the extremity, became very red hot, but the negative end never got even to a dull redness. It was certainly very hot, but never higher than a black heat. I next increased the length of the ends of the wires *exterior* to the circuit; and eventually heated two inches of the positive wire to bright redness; but no such heat took place on the other wire. Thus satisfying myself that I was not mistaken, I called Mr. Mason to come and look at it: and after satisfying that gentleman by an experiment or two, we called Mr. Gassiot and Mr. Walker to come and witness the novel phenomenon. We now changed the places of the polar wires, making that positive which before had been negative, &c. Still the positive wire showed the same fact. You will easily understand that I experienced a great degree of pleasure at the appearance of this beautiful fact, which seemed to demonstrate the justness of the hypothesis I had so long formed. *No two bodies can be in the same place at the same time*, is an old axiom in philosophy. Hence the blacksmith is enabled to heat his iron rod or nail, by compressing the calorific matter; the blows of his hammer forcing it from the *cavities* into the *particles* of the metal. Thus, also, the electric fluid forces the calorific matter from its natural lodgings in the conductor, and drives it on even to beyond the electrical stream, to take refuge, in a compressed form, in the extremity of the positive wire. Nothing can be more simple to explain; nor do I know of an experiment that tends more to support the doctrine of *one species* of electric matter only; and that it runs through the voltaic conducting wires, *from the positive to the negative pole*.

"To produce the phenomenon I have been describing, requires an extensive series of pairs; certainly not less than one hundred and twenty, but two hundred would answer much better, as much depends upon the play of the fluid between the wires; and I think that the battery is quite as well when not highly charged. I have mentioned one hundred and twenty as the shortest to insure success,

although it is possible that one hundred might show the fact."

The following remarks, in answer to inquiries made of Mr. Sturgeon as to his views regarding the best forms of galvanic batteries, are worth preserving, as the conclusions of so experienced an experimenter, and the more so as they coincide generally with the views of Dr. Hare, and of other distinguished men in this country.—*Eds.*

Form and size of Galvanic Batteries.

"With respect to galvanic batteries, we can never expect to find one which will exhibit every class of phenomena to the best advantage. The pile, with moistened card board in pure water, or a well constructed Cruikshank, charged with water, answers best for charging Leyden jars, deflections of pith balls, &c. And the more extensive the series the better. The size of the plates has also much to do in this business. A single pair of plates, charged with dilute nitrous acid, answers best for most electro-magnetic experiments. For a display of *brilliant* calorific phenomena, the burning of charcoal, deflagration of laminated metals, &c., a series of not less than a hundred pairs answers better than any smaller series. Here again, the size of the plates should never be less than four inches square. Six inch plates answer much better, and two hundred better than one hundred, &c. And these may be either of the Cruikshank form, or of any other, observing that the action with the former is of much shorter duration than with the Wollaston form, and shorter with the Wollaston than with the battery before described.

"Then again, for heating of thick wires, a series of ten or less, of *large plates*, are better than more extensive series.

"For chemical decompositions, there is, perhaps, no battery known so well adapted for them as the jars which I have described. Their sustaining power is a great recommendation. The extent of series will necessarily vary with the nature of the compound operated on. We have found that a series of twelve jars gives a sufficient intensity for the decomposition of acidulated water, (water 10, sulphuric acid 1, or even much less.) Twenty-four jars in a double series of twelve, give about twice as much gas as a single series of twelve. But twenty-four jars in a single series do not give so much gas as when they form a double series of twelve. Again, thirty-six jars in one series, do not give so much gas as when they are formed into a treble series of twelve. Hence a series of twelve of *these* jars seems to be about the best *unit of intensity* for acidulated water. Other compounds will require other *units of intensity* to produce maximum effects—and other batteries will require different extent of series to produce the same *unit of intensity* as that produced by the jars."—*Letter to Prof. Silliman.—(From Silliman's Journal.)*

BRITISH ASSOCIATION PROCEEDINGS AT GLASGOW, 1840.

GENERAL MEETING.—THURSDAY, SEPTEMBER 24.

In consequence of the absence of the Rev. Vernon Harcourt, the President of the past year, the Marquis of Northampton took the chair. He lamented the unavoidable absence of Mr. Harcourt, who had taken so active a share in the formation of the Society, and had been one of its most zealous supporters. He congratulated the Association on assembling in a city equally remarkable for its extensive commerce and great manufacturing industry, and the seat of an ancient university, which had rendered eminent service to the united cause of literature, science, and humanity. Glasgow, the native town of Watt, had taken the lead in the practical application of steam as a moving power, and the animating display of steamers on the Clyde united the triumphs of art to the most romantic scenery of nature. He felt great pleasure in introducing their new President, the Marquis of Breadalbane; and, after a brief reference to the services which the Association had rendered to science, he resigned the chair.

The Marquis of Breadalbane, on taking the chair, stated his sense of the honour conferred on him, and observed, that it was unthought of, and unlooked for on his part; and he was afraid he had no claim to it, save that of one who had a firm conviction of the vast importance and value of science, and an earnest wish to support its best interests by every means in his power. It was unnecessary, he observed, in such a meeting, composed as it was of some of the greatest ornaments of our own country, and many of the highest character in science in foreign countries, to dilate on that bond of union which it presented for promoting the great object—the investigation of truth. The British Association had conferred great and valuable benefits upon the nation, and even the world at large. He adverted to the propriety of such a meeting being held in Glasgow, a city combining in itself more perhaps than any other in the empire, the elements of national wealth—commerce and manufactures. He then called on Mr. Murchison to read—

The Address of the General Secretaries.

In entering upon the duty assigned to us, we heartily congratulate our associates on this our second assembly in Scotland. As on our first visit, we were sustained by the intellectual force of the metropolis of this kingdom, so now, by visiting the chief mart of Scottish commerce, and an ancient seat of learning, we hope to double the numbers of our northern auxiliaries.

Supported by a fresh accession of the property and intelligence of this land, we are now led on by a noble Marquis, who, disdaining not the fields we try to win, may be cited as the first Highland chieftain who, proclaiming that knowledge is power, is proud to place himself at the head of the clans of science.

If such be our chief, what is our chosen ground? Raised through the industry and genius of her sons, to a pinnacle of commercial grandeur, well can this city estimate her obligations to science! Happily as she is placed, and surrounded as she is by earth's fairest gifts, she feels how much her progress depends upon an acquaintance with the true structure of the rich deposits which form her subsoil; and, great as they are, she clearly sees that her manufactures may at a moment take a new flight by new mechanical discoveries. For she it is, you all know, who nurtured the man whose genius has changed the tide of human interests, by calling into active energy a power which (as wielded by him), in abridging time and space, has doubled the value of human life, and has established for his memory a lasting claim on the gratitude of the civilized world. The names of Watt and Glasgow are united in imperishable records!

In such a city then, surrounded by such recollections, encouraged by an illustrious and time-honoured University, and fostered by the ancient leaders of the people, may we not augur that this meeting of the British Association shall rival the most useful of our previous assemblies, and exhibit undoubted proofs of the increasing prosperity of the British Association?

Not attempting an analysis of the general advance of science in the year that has passed since our meeting at Birmingham, we shall restrict ourselves, on the present occasion, to a brief review of what the British Association has directly effected in that interval of time, as recorded in the last published volume of our Transactions. From this straight path of our duty we shall only deviate in offering a few general remarks on subjects intimately connected with the well being and dignity of our Institution.

One of the most important—perhaps the most important service to science—which it is the peculiar duty of the Association to confer, is that which arises from its relation to the Government—the right which it claims to make known the wants of science, and to demand for them that aid which it is beyond the power of any scientific body to bestow. In the fulfilment of this important and responsible duty, the Association has continued to act upon the principle already laid down in the Address of the General Secretaries at the Meeting at Newcastle in 1838, namely, to seek the aid of Government in no case of doubtful or minor importance; and to seek it only when the resources of individuals, or of individual bodies, shall have proved unequal to the demand. The caution which it has observed in this respect has been eminently displayed in the part which it has taken with reference to the Antarctic expedition, and to the fixed Magnetical Observatories. It abstained from recommending the former to the Government until it had

called for and obtained from Major Sabine, by whom the importance of such an expedition was first urged, a report in which that importance was placed beyond all doubt; and it withheld from urging the latter, although its necessity was fully felt by some of its own members, until the letter of Baron Humboldt to the Duke of Sussex gave authority and force to its recommendation.

The delay which has in consequence occurred, has been productive of signal benefit to each branch of this great twofold undertaking. Since the time alluded to, our view of the objects of investigation in terrestrial magnetism have been greatly enlarged, at the same time that they have become more distinct. Major Sabine's memoir on the Intensity of Terrestrial Magnetism, has served to point out the most interesting portion of the surface of the globe as respects the distribution of the magnetic force, and has indicated, in the clearest manner, what still remained for observation to perform: and the beautiful theory of M. Gauss, which has been partly built upon the data afforded by the same memoir, while it has assigned the most probable configuration of the magnetic lines of declination, inclination, and intensity, has done the same service with respect to all the three elements.

In another point of view, also, delay has proved of great value to both branches of the undertaking, but more especially to the fixed observatories. Our means of instrumental research have, since the time of their first projection, received great improvements, as well in their adequacy to the objects of inquiry, as in their precision; and finally, the two great lines of inquiry—the research of the distribution of terrestrial magnetism on the earth's surface, and the investigation of its variations, secular, periodic, and irregular,—have been permitted to proceed *pari passu*.

Last of all, the prudent caution, and vigilant care, which the two great scientific bodies have exhibited, both in the origin and progress of the undertaking, have naturally inspired the government with confidence; and while on the one hand science has not hesitated to demand of the country all that was requisite to give completeness to a great design, so on the other, the government of the country has not hesitated to yield, with a liberal and unsparing hand, every request the importance of which was so well guaranteed.

But while we thus enumerate the benefits which have resulted to magnetical science from the delay, it must be also acknowledged that something has been lost also, not to science, but to British glory. Although terrestrial magnetism stood forward as the prominent object of the Antarctic expedition, yet it was also destined to advance our knowledge of the "*physique du globe*," in all its branches, and especially in that of geography. Had the project of an Antarctic expedition been acceded to when it was first proposed, viz., at the meeting of the British Association in Dublin, in 1835, there can be no reasonable doubt, that a discovery, which, by its extent, may almost be designated a Southern Continent, situated in the very region to which its efforts were to have been

chiefly directed, must have fallen to its lot; and the flag of England been once more the first to wave over an unknown land. But while, as Britons, we mourn over the loss of a prize which it well became Britain and British seamen to have made their own, it is our part too as Britons, as well as men of science, to hail the great discovery—one of the very few great geographical discoveries which remained unmade;—and to congratulate those by whom it has been achieved, those whom we are proud to acknowledge as fellow-labourers, and who have proved themselves in this instance our successful rivals in an honourable and generous emulation.

The caution which has characterized the British Association in the origination of this great undertaking, has been followed up by the Royal Society in the manner in which it has planned the details, and in the vigilant care with which it has watched over the execution. Of the success which has attended this portion of the work, the strongest proof has been already given in the unhesitating adoption of the same scheme of observation by many of the continental observers, and in the wide extension which it has already received in other quarters of the globe. All that yet remains is to provide for the speedy publication of the results. The enormous mass of observations which will be gathered in, in the course of three years, by the Observatories established under British auspices, and by the Antarctic expedition, will render this part of the task one of great expense and labour. To meet the former, we must again look to the Government, and to the East India Company, who will certainly not fail to present the result of their munificence to the world in an accessible form. The latter can only be overcome by a well organized system. The planning of this system will, of course, be one of the first duties of the Royal Society; and it is important that it should be so arranged, that while every facility in the way of reduction may be given to those who shall hereafter engage in the theoretical discussion of the observations, care is taken at the same time that the data are presented entire, without mutilation or abridgment. The Council of the Royal Society will, doubtless, be greatly assisted in this duty by the eminent individual who has had in every way so large a share in the formation of these widely scattered magnetic establishments, and whose own Observatory, founded by the munificence of the Dublin University, has nearly completed twelve months' magnetic observations on that enlarged and complete system of which it set the first example.

In referring, as we have done, to those most valuable services which the Royal Society have rendered, and are continuing to render, in directing and superintending the details of this great undertaking in both its branches, it is right that, on the part of the British Association, we should express the cordial satisfaction and delight with which we witness their exertions, united with our own in this common cause; nor should we omit to recognize how much this desirable concurrence has been promoted by the influence of the noble President of the Royal Society, the Marquis of Nor-

thampton, whom, as on so many former occasions, we have the pleasure of seeing amongst us, as one of our warmest supporters and most active members.

In the volume of our Transactions now under notice, is contained the memorial presented to Lord Melbourne by the Committee of the British Association, appointed to represent to her Majesty's Government the recommendation of the Association on the subject of terrestrial magnetism. This memorial is one of many services which have been rendered to our cause, by Sir John Herschel, whose name, whose influence, and whose exertions, since our meeting two years since at Newcastle, have largely contributed to place the subject where it now stands. The devoted labour of other of our members has long been given to an object which they have had deeply at heart, viz. the advancement of the science of terrestrial magnetism; but the sacrifice which Sir John Herschel has made of time, diverted from the great work in which his ardent love of astronomy, his own personal fame, and his father's memory, are all deeply concerned, the more urgently demands from our justice a grateful mention,—because the science of magnetism had no claim on him, beyond the interest felt in every branch of science, by one to whom no part of its wide field is strange, and the regard which a national undertaking such as this deserved, from the person who occupies his distinguished station amongst the leaders of British science.

The advancement of human knowledge, which may be reckoned upon as the certain consequence of the Antarctic expedition (should Providence crown it with success), and of the arrangements connected with it, is of so extensive a nature, and of such incalculable importance, that no juster title to real and lasting glory than it may be expected to confer, has been earned by any country, at any period of time; nothing has ever been attempted by England more worthy of the place which she occupies in the scale of nations. When much which now appears of magnitude in the eyes of politicians has passed into insignificance, the fruits of this undertaking will distinguish the era which gave it birth, and, engraved on the durable records of science, will for ever reflect honour on the scientific bodies which planned and promoted it, and on the Government, which, with so much liberality, has carried it into effect.

Were the value of this association, gentlemen, to be measured only by the part which it has taken in suggesting and urging this one object, there might here be enough to satisfy the doubts of those who question its utility: to overlook such acts as these, and the power of public usefulness which they indicate, to scrutinize with microscopic view the minute defects incidental to every numerous assemblage of men, to watch with critical fastidiousness the taste of every word which might be uttered by individuals amongst us, instead of casting a master's eye over the work which has been done, and is doing, at our meetings, is no mark of superior discernment and comprehensive wisdom, but is evidence rather of confinement to narrow views, and an indulgence of vain and ignoble passions.

But to proceed with our useful efforts. One of the principal objects of our annual volumes is the publication in the most authentic form of the results of special researches, undertaken by the request, and prosecuted in many instances at the cost, of the Association. It is a trite remark, that if a man of talent has but fair play, he will soon secure to himself his due place in public estimation. We fully admit the truth of this in many instances, and above all where the points of research are connected with commerce and the useful arts ; but many also are the subtile threads of knowledge, which, destined at some future day to be woven into the great web in which all the sciences are knit together, are yet not appreciable to the vulgar eye, and, if simply submitted to public judgment, would too often meet with silent neglect. Numberless, we say, are the subjects (and if your Association exceeds a centenary, still more numerous will they be) with which the retired and skilful man may wish to grapple, and still be deterred by his want of opportunity or of means. Then is it that, adopting the well-balanced recommendations of the men in whose capacity and rectitude you confide, you step forward with your aid, and bring about these recondite researches, the result of which in the volume under our notice we now proceed to consider.

The first of these inquiries to which we advert, you called for at the hands of Prof. Owen, upon "British Fossil Reptiles," one of the branches of Natural History, on a correct knowledge of which the developement of Geology is intimately dependent.

The merits of the author selected for this inquiry are now widely recognized, and he has, with justice, been approved as the worthy successor of John Hunter, that illustrious Scotchman who laid the foundation of comparative anatomy in the British isles. That this science is now taking a fresh spring, would, we are persuaded, be the opinion of Cuvier himself, could that eminent man view the progress which our young countryman is making towards the completion of the temple of which the French naturalist was the great architect. It is therefore a pleasing reflection, that when we solicited Professor Owen to work out this subject, we did not follow in the wake of Europe's praise, but led the way (as this Association ought always to do), in drawing forth the man of genius and of worth ; and the value of our choice has been since stamped by the approval of the French Institute.

If Englishmen* first perceived something of the natural affinities of Palæosaurians, it was reserved for Cuvier to complete all such preliminary labour. The publication of his splendid chapters on the Osteology of the crocodile and other reptiles, drew new attention and more intelligent scrutiny to these remains ; and it ought to be a subject of honest pride to us to reflect that the most interesting fruits of the researches of that great anatomist were early gathered by the English palæontologists, Clift and Home. One of our leaders, whose report on Geology ornaments the volumes of this

* Stukeley.

Association, formed the genus *Plesiosaurus*, on an enlarged view of the relation subsisting between the ancient and modern forms of reptile life; while shortly after Buckland established the genus *Megalosaurus*, and Mantell, *Iguanodon* and *Hylæosaurus*, worthy rivals of the *Geo Sauri* and *Moso Sauri* of Cuvier. The other Englishmen who have best toiled in the field, are De la Beche, Hawkins, and Sir Philip Egerton.

Yet although this report is on *British* reptiles, we are fully alive to the great progress which this department has made, and is making, on the Continent, through the labours of Count Münster, Jäger, and Hermann Von Meyer. The last-mentioned naturalist has been for some time preparing a series of exquisite drawings of very many forms unknown to us in England, most of which have been detected in the Muschelkalk, a formation not hitherto discovered in the British isles. Yet despite of all that had been accomplished in our own country or elsewhere, Professor Owen has thrown a new light of classification on this subject, founded on many newly-discovered peculiarities of osseous structure, and has vastly augmented our acquaintance with new forms, by describing sixteen species of *Plesiosauroi*, three of which only had been recognizably described by other writers; and ten species of *Ichthyosauroi*, five of which are new to science. Such results were not to be obtained without much labour; and previous to drawing up his report, Professor Owen had visited the principal depositaries of *Enaliosauroi* described by foreign writers, as well as most of the public and private collections of Britain. This, the first part of Mr. Owen's report, concludes with a general review of the geological relations and extent of the strata through which he has traced the remains of British *Enaliosauroi*. The materials which he has collected for the second and concluding portion of his report, on the terrestrial and crocodilean sauria, the Chelonia, Ophidian, and Batrachian reptiles, are equally numerous, and the results of these researches will be laid before the Association at our next meeting. Deeply impressed as we are with the value of this report, we cannot conclude a notice of it without again alluding to its origin, in the words of Professor Owen himself: "I could not," says he, "have ventured to have proposed to myself the British Fossil Reptilia as a subject of continuous and systematic research, without the aid and encouragement which the British Association has liberally granted to me for that purpose."

Mr. Edward Forbes, whose labours in detecting the difference of species and varieties among the existing marine testacea of our shores have been most praiseworthy, has on this occasion given us a report "On the Pulmoniferous Mollusca of the British Isles." The variations in the distribution of the species in this class of animals, are shown by him to depend both upon climate and upon soil, the structure of the country (or geological conditions) having quite as much share in such varied distribution as the greatest diversity of temperature. The Association has to thank the author for valuable tables, which show both the distribution of the pulmo-

niferous molusca in our islands, and their relations to those of Europe generally.

From zoological researches let us now turn to physical geology. One of the most interesting fruits of modern experimental research, is the knowledge of the fact, that electrical currents are in continual circulation below the surface of the earth. Whether these currents, so powerful in developing magnetical and chemical phenomena, are confined to mineral veins and particular arrangements of metal and rock, or generally capable of detection by refined apparatus well applied, appeared a question of sufficient importance to deserve at least a trial on the part of the Association. Our present volume records the result of such a trial on the ancient and very regularly stratified rocks of Cumberland, consisting of limestone, sandstone, shale, and coal, so superimposed in many repetitions as to resemble not a little the common arrangement of a voltaic pile. Varied experiments, with a galvanometer of considerable delicacy, failed to detect, in these seemingly favourable circumstances, any electrical current.

The extensive and rapidly increasing applications of iron to public and private structures of all kinds in which durability of material is a first requisite, have made it highly desirable to possess accurate information respecting the nature of the chemical forces which effect the destruction of this hard and apparently intractable metal. The preservation of iron from oxidation and corrosion, is indeed an object of paramount importance in civil engineering. The Association was therefore anxious to direct inquiry to this subject, and gladly availed itself of the assistance of Mr. Mallet, a gentleman peculiarly qualified for such investigations, both from his knowledge as a chemist, and from his opportunities of observation as a practical engineer. An extensive series of experiments has accordingly been instituted by him, with the support of the Association, on the action of sea and river water, in different circumstances as to purity and temperature, upon a large number of specimens of both cast and wrought iron of different kinds. These experiments are still in progress, and the effects are observed from time to time. They will afford valuable data for the engineer, and form the principal object of the enquiry, but a period of a few years will be required for its completion. In the meantime, Mr. Mallet has furnished a report on the present state of our knowledge of the subject, drawn from various published sources, and from his own extensive observations. In this report he examines very fully the general conditions of the oxidation of iron, and how this operation is greatly promoted, although modified in its results, by sea-water; also in what manner the tendency to corrosion is affected by the composition, the grain, porosity, and other mechanical properties of the different commercial varieties of iron. The influence of minute quantities of other metals, in imparting durability to iron, is also considered. Mr. Mallet devotes much attention to the consequences of the galvanic association of different metals with iron, a subject of recent interest from the applications of zinc and other metals to protect iron, which

are at present agitated. He concludes this, his first report, by recommending a series of enquiries, ten in number, which will supply the desiderata immediately required by the engineer and by the chemist.

We have next to notice a report by Professor Powell, "On the present state of our knowledge of Refractive Indices for the standard rays of the solar spectrum in different media." The difficulty which the fact of the dispersion of light has offered to the universal application of the undulatory theory, has been in a great measure removed by the analysis of Cauchy and others, who have considered the distances of the undulatory particles as quantities comparable to the length of a wave. Velocities of propagation of the different rays of the spectrum, are made to depend upon the length of wave which constitutes a ray of a given colour, and upon certain constants proper to the medium. These constants being obtained from observations on refractive indices for certain definite rays (or dark lines) of the spectrum, the refrangibility of any other definite ray (whose wave-length has been ascertained by examining an interference-spectrum), becomes known, and may be compared with observation as a test of theory; such experiments have been made by Fraunhofer, Rudberg, and Professor Powell, who has given a tabular view of the various results, without, however, instituting the comparison between theory and observation, which it would be desirable to extend further than has yet been done. It would be important also to elucidate the disturbing effect of temperature, which prevents even existing observations from being rigorously comparable.

The calculations respecting the tides, which have been prosecuted by the aid of the Association ever since its institution, have been continued this year by Mr. Bunt, under the direction of Mr. Whewell. These calculations have now reached such a point, that the mathematician, instead of being, as at the beginning of this period, content with the first rude approximations, is now struggling to obtain the last degree of accuracy.

The country in which we are now assembled has always been conspicuous for attention to meteorology, a branch of physical science in which the British Association, with its power of combining the efforts of many observers in distant quarters of the globe, may hope to be especially useful.

In Scotland, Leslie opened a new train of inquiry, by examining the earth's temperature at different depths; and his successor in the University of Edinburgh is now directing, at the request of the Association, a large and complete course of experiments on that interesting subject. Framed in conformity with the plans adopted for similar objects by Arago and Quetelet, these researches of Professor Forbes contain also the means of determining the power of conducting heat, which different sorts of rock possess; and may thus throw light on some of those peculiarities in the distribution of temperature at greater depths below the surface, which have become known by experience, but are not explained by theory.

In Scotland, Sir David Brewster was the first to obtain an hourly

meteorological journal for a series of years, and to draw from that fertile source new and important deductions, which have had a powerful influence on the progress of scientific meteorology. How gratifying to receive, through the same hands, after a lapse of nearly fifteen years, an additional contribution of the same kind, and from the same country ; but embracing new conditions, on a new line of operations, in order to obtain new results. By the observations now in progress at Inverness, and at Kingussie, the influence of elevation in modifying the laws which have been found to govern the hourly distribution of heat near the level of the sea, may be discovered, and thus a great addition be made to the experimental results, for which science has long been grateful to the distinguished philosopher we have named, and which have been described as "of the highest value to meteorology, and as the only channel through which any specific practical information can be obtained in this most interesting department of physics."

This is no ordinary praise. It is the just tribute of one who is worthy to offer it ; one, who at the call of the British Association, has conducted at Plymouth a still more extensive series of similar observations, and has added to them hourly comparisons of the temperature and moisture of the air, and an hourly record of barometric oscillations. Mr. Snow Harris has presented in a few pages of our last report the precious results of 70,000 observations, and thus rendered them immediately available in the foundations of accurate meteorology. The documents thus patiently collected are, however, not yet exhausted in value ; they may be again and again called into the court of science, and made to yield testimony to other, and as yet, unsuspected truths. They must not be lost. Shall we lay them by in manuscript among other unconsulted records of the past labours of men, or by undertaking their publication, do justice to our workmen, and establish a new claim on the imitation of the present, and the gratitude of future days ? This question is of serious import. Already, stimulated by success in thermometric registration, we have set to work on a more perplexing problem ; we have resolved to bind even the wandering winds in the magic of numbers. While we speak, the beautiful engines of our Whewells and Osiers are tracing at every instant of time the displacements of the atmosphere at Cambridge, at Plymouth, at Birmingham, in Edinburgh, in Canada, in St. Helena, and at the Cape of Good Hope ; and, ere long, we may hope to view, associated in one diagram, the simultaneous movements of the air over Europe, America, Africa, India, and Australia, recorded with instruments which *we* have chosen, by men whom *we* have set to work.

Amongst the causes which tend to retard the progress of science, few, perhaps, operate more widely than the impediment to a free and rapid communication of thought and of experiments, occasioned by difference of language. It appeared to the British Association that this impediment might, in some degree, be removed, as far as regards our own country, by procuring, and causing to be pub-

lished, translations of foreign scientific memoirs judiciously selected. Accordingly, at each of the meetings at Newcastle and Birmingham, a grant of 100*l.* was placed at the disposal of a committee appointed to carry this purpose into effect. Aided by the contributions of several translations which have been gratuitously presented to them, the committee have been enabled, in the two last years, to publish fourteen memoirs on subjects of prominent interest and importance in the mathematical and physical sciences, bearing the names of some of the most eminent of the continental philosophers.

Such, gentlemen, is an imperfect review of our recent proceedings. In two essential respects the British Association differs from all the annual scientific meetings of the Continent, no one of which has printed Transactions or employed money in aiding special researches. We also differ from them in the communications which, in the name of the representatives of science, assembled from all parts of the United Kingdom, we feel ourselves authorized to make from time to time to the Government on subjects connected with the scientific character of the nation. On our first visit to Scotland, for example, we felt it to be an opprobrium that this enlightened kingdom should, in one essential feature of civilization, be still behind many of the continental states, and we prepared an address to his late Majesty's Government, urging strongly the necessity of the construction, without delay, of a map of Scotland, founded on the trigonometrical survey. Representations to the same effect have since been made by the Royal Society of Edinburgh, and by the Highland Society, and the subject has now engaged that attention which will, we trust, soon procure for this country the first sheets of a large and complete map.

If, then, it be asked why are the men of highest station happy to associate and mingle with us in official duties? Why have the heads of the noble houses of Fitzwilliam, Lansdowne,* Northampton, Burlington, Northumberland, and Breadalbane, alternated in presiding over us, with our Bucklands, our Sedgwicks, our Brisbanes, our Lloyds, and our Harcourts? Why, indeed, on this very occasion has Argyll himself, overlooking the claims due to his high position, and his ancient lineage, come forward to act with us, and even to serve in a subordinate office? May we not reply, that it is, we believe, a consequence of the just appreciation on the part of these patriotic and enlightened noblemen, of the beneficial influences which this Association exercises in so many ways on the sources of the nation's power and honour.

If we have hitherto dwelt almost exclusively on the value of our transactions, researches, recommendations, and the good application of our finances, let it not, however, be supposed, that we are not also fully alive to the advantages which flow from the social inter-

* The Marquis of Lansdowne, who had accepted the office, was prevented from attending by deep domestic affliction, and the Marquis of Northampton cheerfully supplied his place.

course of these meetings, by bringing together, into friendly communion, from distant parts, those who are struggling on (often remote and unassisted) in advancing experimental science. If, indeed, this principle of union (which we are proud to have borrowed from our German brethren,) has been hitherto found to work so well amongst our own countrymen, we cannot but doubly recognize its value when we see assembled so many distinguished persons from foreign countries. In the presence of these eminent men, we forbear to allude to individual distinctions, conscious that any brief attempt of our own would fall far short of a true estimate of merits, the high order of which is indeed known to every cultivator of science in Britain. Well, however, may we rejoice in having drawn such spirits to our Isle; valuable, we trust, will be the comparisons we shall be enabled to make between the steps which the different sciences are making in their countries and in our own.

That advantages, indeed, of no mean order arise from such social intercourse, is a feeling now so prevalent, that foreign national associations for the promotion of natural knowledge, have rapidly increased. Germany, France, and Italy have their annual assemblies, and our allies of the Northern States hold their sittings beyond the Baltic. In all this there is doubtless much good, but an occasional more extensive intercourse of a similar nature, to be repeated at certain intervals, is greatly to be desired.

It has therefore appeared to us (and we say it after consultation with many of our continental friends, who equally feel the disadvantage), that the formation of a general congress of science might be promoted at this meeting, which, not interfering with any assemblies yet fixed upon, or even contemplated, may be so arranged as to permit the attendance of the officers and active members of each national scientific institution.

If the British Association should take the first step in proposing a measure of this kind, and should solicit the illustrious Humboldt to act as President, we are sure that scientific men of all nations would gladly unite in offering this homage to a man whose life and fortune have been spent in their cause, whose voice has been so instrumental in awakening Europe to the inquiry into the laws of terrestrial magnetism, and whose ardent search after nature's truths has triumphed over the Andes and the Altai.

If such be your suggestion, then will a fresh laurel be added to the wreath of this city. She who, through the power bequeathed to her by her illustrious offspring, conveys with rapid transit her inventions and her produce to the remotest lands, well can she estimate the value of an union of men whose labours can but tend to cement the bonds of general peace. In such a body the British representatives would, we trust, form no unobtrusive band; and with minds strengthened by the infusion of fresh knowledge, they would, on re-assembling for our own national ends, the better sustain the permanent and successful career of the British Association.

Mr. Taylor, the Treasurer, then read the Report of the Receipts and Expenditure for the past year.

Mr. Phillips announced the order of proceedings; and added, that a steamer had been placed at the disposal of the Association, which would convey the members to Arran at six o'clock on Saturday morning; and that the railway proprietors had offered to convey members to and from Ardrossan.

SATURDAY.

“On a new method of Photogenic Drawing,” by Dr. Schafhaeutl.

After some observations on the comparatively low value of all drawings taken by means of the camera-obscura, in an artistical point of view, and on the principal points on which Mr. Talbot's and M. Daguerre's methods of fixing the drawings of the camera-obscura were founded, the author proceeded to describe his peculiar methods of producing photogenic drawings in Mr. Talbot's, that is, in a negative way; then, secondly, he described two new methods of obtaining photographs in a *positive* way. His first method tended to obtain a paper of very great sensibility by a comparatively short process. He recommended Penny's improved patent metallic paper, and spreading a concentrated solution of the nitrate of silver (140gr. to $2\frac{1}{2}$ drachms of fused nitrate to 6 fluid drachms of distilled water), by merely drawing the paper over the surface of the solution contained in a large dish. In order to convert this nitrate of silver into a chloride, the author exposed it to the vapours of boiling muriatic acid. A coating of chloride of silver, shining with a peculiar silky lustre, was by this method generated on the surface of the paper, without penetrating into its mass; and in order to give to this coating of chloride the highest degree of sensibility, it was dried, and then drawn over the surface of the solution of the nitrate of silver again. After having been dried, the paper was ready for use; and no repetition of this treatment was able to improve its sensitiveness. The author's process for fixing definitively the drawing was as follows:—He steeped the drawing from five to ten minutes in alcohol, and, after removing all superfluous moisture by means of blotting-paper, and drying it slightly before the fire, the paper thus prepared is finally drawn through diluted muriatic acid, mixed with a few drops of an acid nitrate of mercury. The addition of the nitrate of mercury requires great caution, and its proper action must be tried first on paper slips, upon which have been produced different tints and shadows by exposure to light; because, if added in too great a quantity, the lightest shades disappear entirely. The paper, after having been drawn through the above-mentioned solution, is washed well in water, and then dried in a degree approaching to about 158° Fahr., or, in fact, till the white places of the paper assume a very slight tinge of yellow. The appearance of this tint indicates that the drawing is fixed permanently. The author's way for reversing the

drawing is, in the principal points, the same as that suggested by Mr. Fox Talbot. In order to obtain a photogenic drawing in a direct and positive way, the author uses his above-mentioned paper, allows it to darken in a bright sunlight, and macerates it for at least half an hour in a liquid, which is prepared by mixing *one part* of the already described acid solution of nitrate of mercury with from nine to ten parts of alcohol. A bright lemon-yellow precipitate, of basic hyponitrate of the protoxide of quicksilver falls, and the clear liquid is preserved for use. The macerated paper is removed from the alcoholic solution, and quickly drawn over the surface of diluted hydrochloric acid (1 part strong acid to 7 or 10 of water), then quickly washed in water, and slightly and carefully dried in a heat not exceeding 212° of Fahr. The paper is in this state ready for being bleached by the rays of the sun; and in order to fix the obtained drawing, nothing more is required than to steep the paper a few minutes in alcohol, which dissolves the free bichloride of mercury. The maceration must not be continued too long, as in that case the paper begins to darken again. The author's second method of producing positive photogenic drawings was by using metallic plates, and covering them with a layer of hydruret of carbon, prepared by dissolving pitch in alcohol, and collecting the residuum on a filter. This, when well washed, is spread as equally as possible over a heated even metallic plate of copper. The plate is then carbonized in a closed box of cast iron, and, after cooling, passed betwixt two polished steel rollers, resembling a common copper-plate printing-press. The plate, after this process, is dipped into the above-mentioned solution of the nitrate of silver, and instantly exposed to the action of the camera. The silver is, by the action of the rays of the sun, reduced into a perfect metallic state, and the lights expressed by the different density of the milk-white deadened silver, the shadows by the black carbonized plate. In a few seconds, the picture is finished; and the plate is so sensitive, that the reduction of the silver begins even by the light of a candle. For fixing the image, nothing else is required, except dipping the plate in alcohol mixed with a small quantity of the hyposulphite of soda, or of pure ammonia.

Professor Graham then gave an Abstract of Professor Liebig's New Chemical Views relative to Agriculture and Physiology, as contained in his Report on the applications of Organic Chemistry in Agriculture and Physiology.

The primary source, it is observed, whence man and animals derive the means of their growth and support, is the vegetable kingdom. Plants, on the other hand, find new nutritive material *only in inorganic substances*. It is obvious, that the last proposition, if true, will afford a firm basis on which to build the superstructure of the chemical physiology of plants. A different opinion has hitherto prevailed. The fertility of every soil has been generally supposed by physiologists to depend on the presence in it of a peculiar substance, to which they have given the name of *humus*. This substance, believed to be the principal nutriment of plants,

and to be extracted by them from the soil in which they grow, is itself the product of the decay of other plants. The obvious difference in the growth of plants, according to the known abundance or scarcity of *humus*, was considered an incontestible proof of the correctness of this opinion. Yet Liebig adduces the most conclusive proofs that *humus*, in the form in which it exists in the soil, does not yield the smallest nourishment to plants. 1st. The humus or humic acid of chemists, (obtained by means of precipitating an alkaline decoction of mould or peat by means of acids,) although soluble, when newly precipitated, is known to become completely insoluble when dried in the air, or when exposed in the moist state to the freezing temperature. This is also demonstrated by treating a portion of good mould with cold water. The fluid remains colourless, and is found to have dissolved less than 100,000th part of its weight of organic matters, and to contain merely the salts which are present in rain water. Decayed wood also yields only slight traces of soluble materials. It has, indeed, been admitted by physiologists, that humic acid, in its unaltered condition, cannot serve for the nourishment of plants; and hence they have assumed that the lime of the different alkalies found in the ashes of vegetables render soluble the humic acid, and fit it for the process of assimilation. But even supposing the humic acid to be absorbed by plants, in the form of that salt, which contains the largest proportion of humic acid, namely, the humate of lime, Liebig shows, from the known quantity of the alkaline bases contained in the ashes of plants, in relation to the carbon they contain, that not so much as 1-30th of the carbon of fir wood, nor so much as 1-20th of the carbon of wheat straw, could be derived from humus in this way. 2nd. Humate of lime requires 2,500 parts of water for solution. Now, supposing all the rain water which falls upon a field to become saturated with humate of lime, and to be absorbed by the plants growing upon it, then the quantity of humate of lime, which the plants thus nourished could obtain, might be calculated. But it proves to be quite insufficient to account for the carbon contained in the corn or in beet-root grown upon the land. 3rd. A certain quantity of carbon is taken every year from a forest or meadow, in the form of wood or hay, and, in spite of this, the quantity of carbon in the soil augments—it becomes richer in humus.

The carbon of plants must therefore be derived from other sources; and as the soil does not yield it, it can only be extracted from the atmosphere. Physiologists, in attempting to explain the origin of carbon in plants, overlook the circumstance that the question is intimately connected with that of the origin of humus. It is universally admitted that humus arises from the decay of plants. No primitive humus, therefore, can have existed, for plants must have preceded the humus. That plants derive the carbon exclusively from the decomposition of carbonic acid, chiefly and often entirely supplied by the atmosphere, is the conclusion to which Liebig arrives. They restore oxygen at the same time to the atmosphere, agreeably to the observation of Priestley, De Saussure, and

others. The decomposition of carbonic acid, it is true, is arrested by the absence of light, and then plants appear to produce and evolve carbonic acid. But then, namely, at night, according to Liebig, a true chemical process commences, in consequence of the action of the oxygen in the air upon the organic substances composing the leaves, blossoms, and fruit. This process is not at all connected with the life of the vegetable, because it goes on in a dead plant exactly as in a living one. The formation of acids is effected during the night by a true process of oxidation; the volatile oils also change into resins by the absorption of oxygen. The carbonic acid, which has been absorbed by the leaves and by the roots, together with water, ceases to be decomposed on the departure of day-light: it is dissolved in the juices, which pervade all parts of the plant, and escapes through the leaves by evaporation. Plants which live in a soil containing humus, exhale much more carbonic acid during the night than those which grow in dry situations—the decomposition of the humus in the soil affording additional carbonic acid to the roots of the former. The opinion is not new that the carbonic acid of the air serves for the nutriment of plants, and that the carbon is assimilated by them, having been advocated by the ablest natural philosophers, but has not been properly appreciated by naturalists—partly, Liebig believes, from their imperfect acquaintance with chemistry, and partly from certain objectionable experiments which were instituted by them in order to decide the point. That the development of the plants growing from seeds sown in pure Carrara marble and in sulphur did not advance far, although sprinkled with carbonic acid water, is not to be wondered at, seeing that many conditions are necessary for the life of plants; those of each genus requiring special conditions, and should but one of these be wanting, although all the rest be supplied, the plants will not be brought to maturity. The sources of the nitrogen and earthy bodies, which all plants contain, were withheld in these experiments. The mere observation of a wood or a meadow, Liebig considers infinitely better adapted to decide the question, than all the trivial experiments under a glass globe. Having shown that the carbon of plants is derived from the atmosphere, Liebig next inquires what power is really exerted on vegetation by the humus of the soil.

Woody fibre, in a state of decay, is the substance called *humus*. This body possesses the property to convert oxygen into carbonic acid. A substance then remains, *mould*, which is the product of the complete decay of woody fibre. It constitutes the principal part of all the strata of brown coal and peat. *Humus is a continued source of carbonic acid*, which it emits very slowly. Such is the chief function which Liebig ascribes to it in vegetation. There is no reason to believe that humus, if absorbed by plants, would not be assimilated, more than sugar, starch, and gum, which humus considerably resembles, and which, when absorbed by the roots of plants, are not assimilated, but again discharged by the roots, or excreted by the leaves. Cultivation is useful, as tilling and loosen-

ing the soil allows access of air to the humus, and thus gives rise to the formation of carbonic acid. When a plant is quite matured, and when the leaves, the organs by which it obtains food from the atmosphere, are formed, the carbonic acid of the soil is no further required.

The Assimilation of Hydrogen.—The solid part of plants (woody fibre) contains carbon and the constituents of water ($C+H,O$), or the elements of carbonic acid, together with a certain quantity of hydrogen. The wood may be formed from a combination of the carbon of the carbonic acid with the elements of water, under the influence of solar light, the oxygen of the carbonic acid being at the same time evolved. Or,—and this view Liebig thinks more probable,—plants, under the same circumstances, may decompose water, the hydrogen of which is assimilated along with carbonic acid. The oxygen disengaged from plants will therefore come from water. But the volume of this gas set free would be the same, whether derived from the decomposition of carbonic acid or of water. A part, or the whole of the oxygen besides contained in the carbonic acid, must also be set free, in the formation of such a substance as an essential oil, which contains only a small portion of oxygen, or no oxygen, as a constituent.

On the Origin and Assimilation of Nitrogen.—Prof. Liebig established the fact that the third of the organic elements is uniformly derived by plants from ammonia. Like water, that body admits of numerous transformations in contact with other bodies. He has demonstrated the existence of ammonia in the atmosphere, by original experiments, having obtained it in a minute but sensible quantity from rain water collected at a distance from all habitations. The diffusion of this substance in the mineral kingdom is also evinced by the existence of calcareous nitre soils and rocks, there being good reason to consider nitric acid as a product of the transformation of the former. A salt of ammonia also sublimes with the boracic acid, condensed in the hot boracic lagoons of Tuscany. Ammonia is also observable in the state of a salt in the juices of plants. The juices of the maple-tree and of beet-root are found, in the process of preparing sugar from them, to contain ammonia in considerable quantities. Putrified urine contains nitrogen in the forms of carbonate, phosphate, and lactate of ammonia, and in no other form. It is employed in Flanders as a manure with the best results. Animal manure, Liebig believes to act only by the formation of ammonia. The latter substance must also form the red and blue colouring matter of flowers. The evident influence of *gypsum* upon the growth of grasses, the striking fertility and luxuriance of a meadow upon which it is strewed, depends only upon its fixing in the soil the ammonia of the atmosphere, which would otherwise be volatilized with the water which evaporates. The ammonia, which is in the state of carbonate, is then decomposed, as in the manufacture of sal ammoniac, and the sulphate of ammonia produced. The advantage of manuring fields with *burned clay* and the fertility of ferruginous soils, which have been considered as

facts so incomprehensible, are explained in an equally simple manner. The true cause is this:—The oxides of iron and alumina are distinguished from all other metallic oxides by their power of forming solid compounds with ammonia. The ammonia is separated from them by every shower of rain, and conveyed in solution to the soil. Powdered charcoal surpasses all other substances in the power to absorb ammonia and other gases, and has been observed to promote vegetation in an extraordinary degree. Decaying wood possesses the same property. Humus, therefore, is not only a slow and constant source of carbonic acid, but is also a means by which the necessary nitrogen is conveyed to plants. Nitrogen, Liebig observes, is found in lichens, which grow on basaltic rocks. Our fields produce more of it than we have given them as manure, and it exists in all kinds of soils and minerals which were never in contact with organic substances. The nitrogen in these cases could only have been attracted from the atmosphere. Carbonic acid, water, and ammonia, contain the elements necessary for the support of animals and vegetables. The same substances (he adds) are the ultimate products of the chemical processes of decay and putrefaction. All the innumerable products of vitality resume, after death, the original form from which they sprung. And thus death—the complete dissolution of an existing generation—becomes the source of life for a new one.

But another class of substances is also necessary for the life of vegetables.

The Inorganic Constitution of Plants.—These substances are found in the ashes left after the incineration of plants, although in a changed condition. Many of these inorganic constituents vary according to the soil in which the plants grow, but a certain number of them are indispensable to their developement. Phosphate of magnesia in combination with ammonia is an invariable constituent of the seeds of all kinds of grasses. Plants also contain various organic acids, all of which are in combination with bases, such as potash, soda, lime, or magnesia. Of the different alkaline bases found in plants, Liebig finds reason to conclude, that any one may be substituted for another, the action of all being the same. But the number of equivalents of these various bases remains the same. The analysis of Berthier and Saussure show that the nature of a soil exercises a decided influence on the quantity of different metallic oxides contained in the plants which grow upon it: that magnesia, for example, was contained in the ashes of a pine-tree, grown at Mont Breven, while it was absent from the ashes of a tree of the same species from Mont La Salle, and that even the proportion of lime and potash was very different. But although the composition of the ashes of these pine-trees was so very different, they contained an equal number of equivalents of metallic oxides; or, what is the same thing, the quantity of oxygen contained in all the bases was in both cases the same—being expressed by the number 9.01 in one case, and by 8.95 in another, a coincidence which had escaped the notice of the analyst himself. It is certain that particular acids enter

into different vegetables, and are necessary to their life; some alkaline base is also indispensable, in order to enter into combination with the acids, which are always found in the state of salts. The perfect developement of a plant is therefore dependent on the presence of alkalies or alkaline earths, and its growth is arrested when these substances are totally wanting, and impeded when they are only deficient. Hence it is that of two kinds of tree, the wood of which contains unequal quantities of alkaline bases, one may grow luxuriantly in several soils, upon which the other can scarcely vegetate. Thus 10,000 parts of oak-wood yield 250 parts of ashes, and the same quantity of fir-wood only 83 parts. Hence, firs and pines find a sufficient quantity of alkalies in granitic and barren sandy soils, in which oaks will not grow. Liebig supplies various additional illustrations of the influence of the alkaline metallic oxides on vegetation, amply sufficient to place beyond controversy these conclusions, so important to agriculture and to the cultivation of forests. One of these Professor Graham quoted: a harvest of grain is obtained every thirty or forty years from the soil of the Luneburg heath, by strewing it with the ashes of the heath plants which grow on it. These plants, during the long period mentioned, collect the potash and soda from the decomposing minerals of the soil, which are conveyed to them by rain water; and it is by means of these alkalies that oats, barley, and rye, to which they are indispensable, are enabled to grow on this sandy heath. The supposition of alkalies, metallic oxides, or inorganic matter in general being produced by plants, is entirely refuted by such well authenticated facts. It is thought very remarkable, that those plants of the grass tribe, the seeds of which furnish food for man, follow him like the domestic animals. But none of our corn plants can bear perfect seeds, that is, seeds yielding flour, without a large supply of phosphate of magnesia and ammonia, substances which they require for their maturity. Hence these plants grow only in a soil where these three constituents are found combined, and no soil is richer in them than those where men and animals dwell together. Professor Liebig then applies these great fundamental principles, in this report, to the *art of culture*, under the following heads: use of humus—nutrition and growth of plants—necessity of azotized substances—influence of the food on the produce—composition of soils—the fertility of soils—fallow. Then, under the head of interchange (rotation) of crops and manure, he discusses the varieties and applications of particular manures, composition of animal manures, the essential elements of manure, bone manure, manure supplies nitrogen, mode of applying urine, value of human excrements. In the second part of his report Professor Liebig discusses the chemical processes of fermentation, decay, and putrefaction, under the heads of chemical transformations—eremacausis or decay—vinous fermentation—wine and beer—decay of woody fibre—on the mouldering of bodies—and on poisons, contagious matter, and miasma. The novel theoretical views with which this department of the work abounds are remarkable, equally with those of the preceding part, for their profundity and for their valuable applications. The subjects discussed, however, are numerous, and of such a nature that great injustice would necessarily be done to them in a short and hasty abstract.

Dr. Gregory stated, that having studied Professor Liebig's work, it appeared to him in the highest degree important, as being the first attempt to apply the newly-created science of Organic Chemistry to Agriculture; that, in his opinion, from this day might be dated a new era in that art, from the principles established, with such profound sagacity, by Professor Liebig. He was also of opinion, that the British Association had just reason to be proud of such a work, as originating in their recommendation.

MISCELLANEOUS ARTICLES.

On Beet Sugar ; by J. C. BOOTH.

There are few subjects which have created more sensation in the greater part of Europe, and the United States, simultaneously, than the manufacture of sugar from beet root. That it should have induced many individuals in this country to experiment, with a view to its manufacture, the characteristic enterprise and ingenuity of our people might guarantee, but may we not assign as the chief reason of their failure, or only partial success, the fact, that too many of us still boast of our practical knowledge, with a sidelong sneer at the assistance of science. It is rather more surprising, to observe the intense and all pervading interest manifested on various parts of the continent of Europe, especially in Germany, on the sugar-beet and its important product, as it clearly shows that this learned people have received an impulse with the rest of the world, relative to more modern manufactures, or rather that the zeal with which scientific men have devoted themselves to the advancement of the arts, is now developing its effects on the mass of the community. The frequent questions asked relative to the making of beet-sugar, may be better answered by a concise description of the superior method of extracting sugar from the dried beet, the main part of the account, being taken from Dinger's Polytechnic Journal, for 1838, Bd. LXIX. The drawings in all their details will be omitted, and merely the general features of the process described.

1. *Cleansing*.—They must be washed, to free them from the soil which adheres to them, and this may be executed in a simple tub, or on a larger scale, a vat, into which water flows. A convenient arrangement for this purpose, might be a net-work cylinder, slightly declining from a horizontal position, revolving under water, or through which water should abundantly flow. The beets coming out from the depressed end of the cylinder, will be perfectly clean.

2. *Cutting*.—"They are next cut by a machine into long strips, exhibiting a square by a cross section, i. e. into long parallelepipeds, which is accomplished by a series of small knives attached to a sheet of iron, parallel to, and at short distances from each other, which first make incisions as deep as the required thickness of the pieces, and are followed by a long knife, behind and at right angles to the smaller ones,

by which the strips are separated." The knife with the smaller ones, cut by a vertical motion, but there might be a greater economy of time, by bringing a series of these cutters on a wheel, and attaching the smaller knives to the large one, suffering them to project a little below it, so that their incisions may be immediately followed by the edge of the long knife.

3. *Drying*.—"Various and simple arrangements have been devised, for drying the pieces thus cut, in all of which the principle consists in exposing them in thin layers to a current of air, heated, to a temperature of between 100°. and 145°. Fah.; for if below 100°. they are apt to ferment, and if above 145°. they are liable to decomposition. For this purpose they are placed on wire nets, in the form of drawers, to the depth of one or two inches, the drawers sliding in one over another, at the distance of three inches, to allow a free circulation of air. The drying chamber, or house, is heated, either by a hot air furnace, or by steam tubes. A better arrangement, however, and one requiring but little hand-labour, is a series of endless wire-nets, one over the other, and each passing around a roller at each end. The pieces are carried from the cutting machine, on an endless cloth, up the highest of the nets, on which they fall, and are carried to the farthest end, by its constant motion, where they fall on the next lower endless net, which at this end projects beyond the uppermost, moving in the opposite direction on to the farther end of the second, which does not reach as far as the third, they are received on the latter, and again transported to its farther end, and thus, by moving alternately in opposite directions, on the adjoining nets, they reach the lowest, from which they are thrown off in a dried, or sufficiently dried, state for use. These parallel nets are all in a chamber, heated by steam tubes from below; lower openings in the apartment admitting cold and dry air, the upper ones above the nets, permitting the egress of the hot air, surcharged with vapour. After drying they are ground to powder."

4. *Sugar Extraction*.—"The saccharine matter may be extracted by pure water, but it is found to be more advantageous to add acid or lime to it. The former is preferable, and sulphuric acid the most convenient. To nine parts of water add two-thirds or three-fourths of a pr. ct. of sulphuric acid, (according to the amount of sugar in the beets,) and stir in four, or even more, parts of the powdered beet. The stirring should be continued until the acidulated water is absorbed, when the mass is subjected to pressure in bags; the remaining mass is again treated with the same quantity of equally acid water, and pressed, but the liquid thus obtained, is used for the next fresh quantity of powder. The moistening and pressing are continued until all the sugar is extracted.

The portion first pressed out, is treated with a quantity of slaked lime, a little more than is sufficient to neutralize the acid, and the precipitation of sulphate of lime takes place fully at the temperature of 165°. to 190°. Far. The clear liquid is drawn off and crystalized by the ordinary sugar-refining process."

5. *Theory.*—Beside sugar, there are many other vegetable principles contained in the beet, of which gluten and albumen are the most injurious and difficult of management, but by drying they are rendered insoluble, and cease to be troublesome. It is also in consequence of the same operation, that less animal charcoal is required for purifying the sirop, than where the beets were not dried. Sulphuric acid renders more insoluble the gummy matter, and probably decomposes a combination of a portion of the sugar, but as there are other acids also present, a little more lime is added to neutralize them, than is sufficient for saturating the sulphuric acid. The beet may contain from six to twelve pr. ct. of sugar, but much of it is lost in the process of manufacture. It is similar to that obtained from the sugar-cane, and is hence called cane-sugar, to distinguish it from other varieties, as raisin or starch-sugar, sugar of milk, &c.

On Raisin Sugar ; by J. C. BOOTH.

When raisins have been exposed to the air for a length of time, small crystalline grains will be found upon and within them, which have a sweet taste, and are a species of sugar. The same kind may be made by the action of diastase, or sulphuric acid, on starch, and indeed starch-sugar, or rather starch-sirop is much used in parts of France and Germany. The process of manufacture is as follows :—

1. *Conversion of the Starch.*—One thousand parts of water are brought to the boiling point, in an open vessel of copper or lead, and fifteen parts of sulphuric acid added, previously diluted with thirty parts of water. When the fluids are well mingled, a cover is put on the vessel with a small opening in the centre through which the starch is introduced. Four hundred and fifty to five hundred parts of dry starch, (or as much wet as contains that quantity,) are put into the opening in the cover of the vessel, in very small portions at a time, so that the fluid may continue boiling, and not become thick. A few minutes after the last portion is added, the fire is extinguished, and chalk is thrown in to neutralize the acid. The clear liquor is drawn off, when the sulphate of lime has deposited and filtered through ordinary sugar filters. It is then evaporated to one half its volume, twenty-five parts of animal charcoal stirred in with a little blood, boiled and filtered through Taylor's filtering apparatus. This is starch-sirop, from which sugar may be obtained, by evaporating to 40—45°. Baumé and cooling. It forms a white, coarsely granular mass, from which the molasses may be separated in the ordinary manner. One hundred parts of dry starch, give one hundred and fifty parts of syrup, or about one hundred of dry sugar.

2. *The Theory of the Process.*—The conversion of starch into sugar, by this process, is one of the most singular operations of chemistry, and has given rise to a new doctrine in the science.

We perceive that by the operation on a large scale, they obtain an amount of sugar equal to that of the starch, but De Saussure obtained in a careful experiment, from one hundred of starch, one hundred and eleven of dry sugar. The sulphuric acid is unchanged, for there is the same amount remaining after the operation that was originally introduced ; nothing is absorbed from the air, nor is there any evolution of gas, for the operation may be conducted equally well in closed, or in open vessels. The starch alone has changed, and this change is effected by its taking up a certain quantity of water, or rather the elements of water, hydrogen and oxygen. According to Saussure, one atom of starch takes up about two atoms of water. It appears then that the presence of sulphuric acid is sufficient to produce such an alteration among the elements of starch, that a new and different product results. For this reason and from many analagous facts, the French chemists give to this singular method of decomposition the name of *presence* ; Berzelius calls it *catalysis*, which signifies a decomposition by the interchange of the elements of a body among each other. The catalytic influence of sulphuric acid then, is to convert starch into sugar, where water is present. All other acids will produce the same result, and the same kind of sugar may be obtained in a similar manner from other organic substances, such as linen, cotton, wood, &c. But the change is not immediate, for it is observed to convert the starch first into gum, and the gum into sugar. It is, however, not the mineral acids alone, that produce this effect, for an organic substance has been discovered in malt, which possesses the same power in a higher degree. This is diastase, which converts starch into gum (or dextrine) at a temperature of 150°. to 160°. while the mineral acids require 185°. to 205°. One part of diastase will change two thousand parts of starch into dextrine, and at least one thousand parts into sugar, Through the presence of diastase, therefore, or more properly by its catalytic influence, starch of wheat, potato, &c., is first changed into dextrine, and then into sugar ; a highly interesting fact, as giving us a clearer view of the formation of gum and sugar in plants, and of the processes for manufacturing alchoholic liquids, which require the presence or formation of sugar, prior to their vinous fermentation.

In concluding the above articles on the manufacture of two varieties of sugar, the following table of the amount of sugar consumed in Europe, in 1836, may not be uninteresting. It is extracted from Dingler's Polyt. Jour. lxvii. p. 319.

Kingdoms of Europe.	No. of millions of		lbs. for one individual.
	Inhabitants.	Sugar lb.	
England	16 $\frac{1}{4}$	321 $\frac{1}{4}$	20
Ireland	8	32	4
France	33	178 $\frac{1}{2}$	5 $\frac{1}{3}$
Prussia	14	56	5
Bavaria	4	10	2 $\frac{1}{2}$
Switzerland	2	12	6
Belgium	4	60	15
Holland	2 $\frac{1}{2}$	35	14
Denmark	2	10	5
Sweden and Norway	4	12	3
Spain	14	87	6 $\frac{1}{4}$
Portugal	3 $\frac{1}{2}$	16 $\frac{1}{2}$	5
Smaller German states	8	40	5
Italy	18	36	2
Austria in the commercial union	19	40	2
Austria without commercial union	15	25	1 $\frac{1}{2}$
Russia	40	40	1
	207 $\frac{1}{4}$	1011 $\frac{1}{2}$	

Journal of the Franklin Institute.

Copal Varnish.

The following method of preparing a copal-varnish, is not novel, but its simplicity and the superior quality of the product, may render it acceptable to many of the readers of the journal.

Enclose coarsely-powdered copal in a linen rag, and hang it in the neck of a flask, or bottle, to such a depth that it cannot touch the spirits of wine, which is in the bottom of the vessel. Tie a piece of bladder over the mouth of the flask, and make a few perforations with a pin, for the escape of a little alcoholic vapour. If the vessel be placed in a warm situation, thick and viscid drops of the copal, combined with alcohol, will slowly fall into the liquid below, and gradually dissolve, until the whole of the copal is extracted. When dissolved, the clear liquor may be decanted from a very small quantity of sediment, and it will prove a more transparent and beautiful varnish than can be procured by any other method. The same process is applicable to other difficultly soluble resins, and will be found useful where rapidity is not required.

Ibid.

Soda Manufacture in Hungary.

Native carbonate of soda is found in greatest abundance in Little Cumania, particularly near Shegedin; it likewise occurs in many other places, in greater or smaller quantity. It effloresces out of

the moist earth, forming a white crust, and in the spring of the year, before sunrise, appears like an extensive covering of snow. With greater care than they now employ, the workmen might readily gather it sufficiently pure for ordinary technical purposes by raking. The whole of the surface is gathered, and sold to the soda manufacturers, who distinguish its quality and richness, by the taste. It is leached in square vats, until the remainder ceases to have a saline taste. The fluid is dark brown, and beside carbonate of soda, contains much sulphate and muriate of soda, humic acid, and other mechanical impurities. It is boiled down in a large sheet-iron pan, to a siropy consistence, transferred to an adjoining pan, and evaporated to dryness under constant stirring. The mass is of a dirty yellow, or brown, with white and black spots. It is gradually heated in a calcining furnace with the access of air, until vapors cease passing off, then fused at a higher temperature, and taken out, when partially cooled. A large portion is employed in the country itself, in the manufacture of soap, the remainder sold as raw calcined soda, as there is no manufacture for crystalizing it. If the demand for it were increased, the production of this salt might be increased to three or four times the present amount, as the country contains numerous soda lakes. Beside Trieste, from which some of the productions of Hungary find their way to the American market, there is a port on the Adriatic, belonging exclusively to that kingdom, whence we might obtain at lower rates, the products of one of the most fertile countries of Europe.

Ibid.

On Galls in the Manufacture of Black Ink.

Blue Aleppo galls are employed in great quantity, in the manufacture of black ink, in consequence of the large amount of tannin they contain, nearly all of which, by a judicious management, is converted into gallic acid. Being greatly superior to oak bark in their content of tannin, they might be substituted for it in the process of tanning leather, were not their high price a serious impediment. They are excrescences on the leaf-stem of the *quercus infectoria*, growing in the Levant, and are produced by the incision of the female gall-wasp. There is, however, another kind of galls, the acorn of the *quercus cerris*, which receive a malformation from the incision of an insect, and produces a substance not unlike the Aleppo galls, but much more irregular, and with bold projecting points. They are found abundantly in Hungary, and the southern provinces of Austria, where they are employed indyeing and tanning, particularly in the latter art. They are known under the name of Knoppeln in Germany, and Galles à l'épine in France, and in the former country, are considered but little inferior to good Aleppo galls. A manufactory has been established at Vienna, for obtaining a solid extract from them, which has been successfully employed in dyeing dark colours, and in tanning. Either the

knopperrn, or their extract, might be obtained at Trieste, and might prove a useful substitute for ordinary galls, whether in dyeing, or in the manufacture of ink.

Ibid.

Assay of Gold.

In the last number of the Journal, a new method of assaying gold, proposed by Lewis Thompson, Esq., is extracted from the London and Edinburgh Philosophical Magazine. It consists in adding to the gold assay-piece, an excess of silver, and then fusing the mass down with the chlorides of silver and of sodium, to remove the base metals. The silver is afterwards separated by nitric acid. "By this plan," says the author, "the tedious process of cupellation is avoided."

It may not be unimportant to some of the readers of the Journal, to be informed, that Mr. Thompson's plan differs from the usual one by cupellation, only in two particulars, in both of which the old process has manifestly the advantage. In this process, lead alone is used to remove the base metals, instead of the two chlorides, and it is simpler, perfectly effectual, and not subject to decrepitation. The second point of difference is that a cupel, composed of bone ashes, is used instead of a crucible; and this cupel possesses the invaluable property of absorbing the oxides of lead, and of the base metals, and leaving a clean button, composed only of gold and silver. In the new process this advantage is not presented, and there will be grains to be separated from the crucible, as after the operation of fluxing; thus adding not only to the labour of the process, but to the uncertainty of the result. We are therefore, led to the conclusion, that the process proposed by Mr. Thompson, is more complicated, more inaccurate, and even more "tedious," than that now in universal use.

Postscript.

Sometime after the above article was communicated, an opportunity was taken of making trial of Mr. Thompson's method of assay, and the results render it proper to modify, in some degree, the above remarks. The gold, the fine silver, and the chloride of silver, were melted together in a small crucible, and the button of gold and silver formed was found to be much more perfect and better insulated than had been expected. Five assays were made, and the results, by the old and new process, expressed in thousandths, were as follows:

No 1, by cupellation,	968	by Thompson's process	968.5
2, "	890	"	889
3, "	936.5	"	936.7
4, "	900	"	900.2
5, "	460	"	460.5

This comparison of the two methods is certainly very satisfactory, the greatest difference being but one thousandth.

In employing Mr. Thompson's process, an evil was observed which had not been anticipated. It is, that a sensible portion of the chloride of silver is volatilized during the fusion, and consequently lost. To show this, the following statements of the first and last assays—being of the finest and basest specimens, are presented. The weight 1000 is equal to between 7 and 8 grains.

No. 1. Gold, with silver	1000 : no copper
Fine silver	2000
Chloride of silver	2700=2037 fine silver

5037

Button of gold and silver after melting, 3088

Loss of silver by the process, =1949

No. 5. Gold with silver	494 + copper 506
Fine silver	1400
Chloride of silver	3000=2260 fine silver

4154

Button after melting, 2580

Loss of silver, 1754

On the whole, though the new process is certainly not so good as that by the cupel, and is not likely ever to replace it where numerous assays are to be made, as at a mint, yet it is certainly better than was supposed when the above remarks were made, and it has the advantage, which is valuable under many circumstances, of not requiring a muffle furnace, or a cupel of bone ashes.

Ibid.

A Description of a New Form of Magneto-Electric Machine, and an Account of a Carbon Battery of considerable energy ; by OLIVER W. GIBBS, member of the Junior Class of Columbia College, N. Y.

It is well known, that if a soft iron bar be wound with insulated wire and caused suddenly to approach and recede from the poles of a magnet, temporary magnetism will be induced in the bar, and an electric current in the wire surrounding it. This fact led to the construction of the magneto-electric machine, the principle of which consists in alternately inducing and destroying magnetism in a bar similarly wound with large wire for sparks and deflagrations, and with small for shocks and chemical decompositions. About eight months since it occurred to me that a more simple machine than those commonly used (and which all I believe resemble that of Saxton) might be constructed. My plan was, to take a bar of soft iron of say an inch in diameter by ten inches long, and to slide upon the middle a disk of brass of two inches radius. This would divide the bar into two parts, upon one of

which is to be wound three or four hundred feet of copper bell wire well insulated, and upon the other and separated from the first by the brass disk, about four times that length of fine wire, say No. 25. If now one extremity of the coarse wire be attached to one pole of the battery, and the communication between the other extremity and the other battery pole be alternately made and interrupted by means of a rasp or toothed wheel, magnetism will be induced and destroyed in the iron bar and consequently an electric current will circulate through the fine wire. The use of the brass disk is to prevent by means of a closed circuit, any immediate induction in the fine from the coarse wire, which would inevitably take place were none interposed, and which would convert the instrument from a magneto-electric to an electro-magnetic machine.

Since the above was devised, an obvious improvement has suggested itself. This is founded upon the fact that magnetism is strongest at the extremities of bodies; and consists simply in dividing the bar into three equal spaces by means of two disks of brass similar in size to the one already described. The central division is then to be wound with the coarse and the two outer or polar divisions with the fine wire, connecting the two outer helices in such a manner that they may form one long wire. The battery current is then to be passed through the coarse wire, and the connection made and interrupted as before by a rasp or other interrupting apparatus. As thus constructed, the instrument would produce effects similar to the common magneto-electric machine when used for shocks or decomposition. If it be desired to produce sparks and deflagrations, it would only be necessary to slide off the coils of fine wire from the poles, and to substitute in their stead others made of coarse wire of shorter length and then transmit and interrupt the current through the central coil as before. We should then have within a much smaller compass, an instrument capable of producing all the effects of the common machine of Mr. Saxton, and by combining a number of such bars we might form in a comparatively small compass a magneto-electric battery of great energy. Some of Dr. Page's beautiful interrupting apparatus might doubtless be used successfully with this instrument. As I have no opportunity to construct the instrument myself, I would suggest the trial, especially of the latter form of apparatus, to any who may be interested in the subject. Should it succeed, its advantage would be its superior cheapness and power, (?) and the little space it would occupy.

About the same time that the above instrument was devised, in looking over the list of substances which are capable of forming a galvanic circle together, I was struck with the much higher electro-negativeness of charcoal than of copper in relation to zinc; there being but six substances between zinc and copper, while there are eleven between zinc and carbon, which, moreover, stands even higher than gold, and next below platina. Besides this, its excellent conducting power seemed particularly to qualify it to act as an electrometer. Accordingly, I was led to consider that it might form an excellent battery with zinc or its amalgam, and mentioned the opinion to Professor Renwick. I was however prevented from experimentally demonstrating its powers, until in the month of March I perceived in one of the foreign journals a short account of a carbon battery which had been successfully tried in

England. I immediately constructed a small battery, consisting of only six pairs of zinc and bituminous coal, and arranged as a *couronne des tasses*. The zinc plates were an inch square, consequently there were only six inches of acting zinc surface; the exciting liquid was diluted sulphuric acid. With this battery pure water was easily and rapidly decomposed, though from not having platina electrodes, and from the want of a voltameter, the gas collected was not measured. This experiment was witnessed by Mr. Schaeffer, assistant Professor of Chemistry in the College. To those who possess batteries of considerable power, I would suggest the employment of some form of carbon for electrodes in the place of platina. I hope soon to be able to present a series of experiments on the relative advantages of copper and carbon, especially in the case of the constant battery.

New York, May 9, 1840.

Electricity in Machinery; by AZARIAH SMITH, jun.

Messrs. Editors,—Having frequently heard persons employed in my father's manufactory at Manlius, N. Y., speak of the development of electricity by particular parts of the machinery, I was led by an article in the American Journal for (July?) 1839, to the examination of the phenomena which furnished me with the following facts; which you will please to publish if they add anything to the light already existing upon this subject.

Upon approaching the machinery referred to, which was connected with the spinning apparatus, and near the centre of the manufactory, I observed fibres of cotton of all lengths up to six inches, extending out in different directions from one end of the spinning frames, and waving as if about to leave their resting place for a band two and a half inches broad, which moved the machinery and connected it with a drum seven feet above; the latter being moved by another drum fifteen feet distant, with which it was connected by a horizontal strap, seven inches in breadth. The two drums were of equal diameter, two feet and eight inches, but the wheel by which the spinning machinery was moved and a free pulley by its side were only eight inches; and consequently made two hundred and eighty-eight revolutions in a minute, while the former made seventy two.

Beneath the horizontal strap, and four feet distant from it, the hair of the persons spinning was observed to be affected in a similar manner with the cotton, all the finer and more flexible fibres standing directly upright. Upon placing small fibres of cotton from one to two feet distant from this strap, they would ascend to it, and adhering to its surface advance with it until within a short distance of the drum around which it passed, when they would fall off and descend to the floor. Occasionally fibres would pass to and fro

between the band and the hand placed near it, and once or twice this latter phenomenon took place through a space of two or three feet.

Upon slipping the narrow band from the wheel moving the machinery to the free pulley by its side, the electrical attraction of both the bands was observed to disappear, and this notwithstanding their motions were the same as before—in a moment, however, it was again manifested upon the spinning machine being set in motion by slipping the back upon the motor wheel. This latter phenomena led to an inquiry into the different circumstances of the band in the two cases, when the idea was suggested that the wheel and the free pulley might be made of materials possessing different conducting power, but this a machinist of the manufactory informed me was not the case, both being made of iron and covered with leather. The friction of the spinning machinery, and of the motor wheel upon its axis, which were present in one, but absent in the other case, was the next difference suggested to account for the change, but as the axes of all parts of the machinery were made of iron and connected with iron frame-work, it was concluded that friction here would have no tendency to accumulate electricity. Upon watching the broad horizontal band at the moment the narrow one was slipped from the motor wheel upon the free pulley, the part of it connecting the upper part of the drums was observed to relax, while that connecting their lower surfaces, from being curved downwards by its weights became proportionally tense. In the first case, the upper part of the band was made tense by the great amount of friction in the machinery which it had to overcome, and of course, the friction of the band upon the drums was increased in the same ratio. But when the free pulley only was turned, the friction to be overcome, and consequently that of the bands, was much diminished; and this increased amount of friction of the bands upon the drums in the first case, is to be referred to as the exciting cause of the electricity.

From this statement you will observe that there was no friction of the bands upon each other as is mentioned in the article referred to above, since the horizontal bands were parallel, and the vertical ones eight inches apart at their nearest approximation. In another part of the manufactory, however, two portions of a band were observed which were crossing and rubbing upon each other, but their friction was attended with no observable electrical effects. At this time however the band was passing around a free pulley; I was therefore led to inquire as to its electrical state during the motion of its machinery, and ascertained that its attractive power for cotton, &c. at such times was as great as in that of the bands already spoken of.

Although these facts do not authorize us to dispute those in Mr. — article, yet they naturally suggest the question whether the electricity in that case was not excited by the friction of the band upon the wheels rather than upon each other, and if so, whether the apparent difference between the bands below their

junction and above was not in reality caused by the application of the jar, in the one case to a tense, and in the other to a relaxed portion of the band.

Not being intimately acquainted with the action of electrical apparatus in different circumstances, I am unable to say whether increased pressure of the whole flap of the common machine upon the cylinder would materially increase the amount of electricity developed, but from the above facts, as well as the nature of the case, I should suppose it would, and if so, the circumstance properly attended to in the construction of electrical machines, would render them, *cæteris paribus*, much more powerful.

Human Fossil, alleged to be Antediluvian.

A discovery of an interesting nature, which, it is said, has recently been made in Belgium, at this moment invites the inspection of the scientific and the curious at a house in Leicester-square. It has been laid down by Cuvier, and received as an axiom in geology, that the bones of the inferior animals alone were to be found in a fossil state, and that those of man were invariably wanting; a theory whose tendency militated against the Mosaic account of the creation. In the science of geology there is consequently no problem whose solution offers greater interest than that which depends on the existence or absence of the human antediluvian fossil. This question has now, to all appearance, been set at rest by the discovery lately made (?) of the fossil remains of a child, which were found embedded in silex, in a chalk quarry at Diehgen, near Brussels. We understand the proprietor of the fossil has requested the attendance of the Marquis of Northampton, and several members of the geological society, to inspect and test it with the most minute scrutiny. The result of this inspection must be decisive of its claims to antediluvian origin. The appearance which it presents is that of the head and trunk of an infant, completely formed, but apparently much compressed. The head is perfect—the nape of the neck, the articulations of the vertebræ, the bones of the throat, the chest, shoulders, and parts of the arms equally so, and the ribs are distinctly visible. The right arm is broken short off by the shoulder; the left, which is unmutilated, adheres to the side, and is sunk into it. The lower extremities are indistinct, being thrown up into a circular mass below the abdomen. From a section of the lower part, which was accidentally made in its discovery, the formation of flint, in which it was preserved, is at once apparent, and on its surface portions of the bones are clearly to be traced.

An Aurora Borealis of considerable magnitude and brilliancy, but attended with no peculiarity, was seen here from seven till eleven o'clock on Monday evening the 19th instant. It consisted principally of a strong, steady light in the northern heavens, with the usual black, foggy nucleus below; and of many fine streamers which were displayed at different times during its appearance. The colour of both streamers and steady light was of a misty white.

W. STURGEON.

Manchester, October.

ANSWERS TO CORRESPONDENTS.

1st. The electro-magnetic engine with the rotating disc can never be an effective one; and we would advise our correspondent not to loose any time in making engines on that principle.

2nd. Magnetic-electrical machines, having soft iron magnets instead of permanent ones, have long been before the public. Our correspondent may see several of them at Watkins and Hill's Establishment, 5, Charing Cross.

3rd. We do not see that Mr. Uriah Clarke has omitted any part of the description of his electro-magnetic carriage, excepting, perhaps, some wheel, or wheel and pinion inside. He has given the *kind* and *extent* of his batteries; and has also stated that "the carriage is propelled by an arrangement of machinery on the reciprocating principle; and this reciprocating principle was previously described in the Annals for July last. (See vol. 5, p. 33. fig. 3, plate 1.) If there be any wheel and pinion in the arrangement, the pinion will be on the axle of the fly wheel, and the wheel in which it works on one of the axles of the travelling wheels. Hence the fly wheel will make more revolutions than the travelling wheel.

The Daguerreotype plates may be viewed to advantage by means of either a convex lens, or a spherical concave mirror. If the plate be held before the mirror, and the eye a little above the plate, the effect is very beautiful.

Wishing at all times to comply with the solicitations of our subscribers, we have now undertaken to write a series of familiar, and we hope, instructive lectures on the various branches of Electricity and Magnetism; including Mechanical-Electricity, Galvanic-Electricity, Voltaic-Electricity, Thermo-Electricity, Magnetic-Electricity, Magnetism, Electro-Magnetism, and Electro-Chemistry.

We are aware, that this undertaking is an arduous task, but being anxious to give every assistance in our power to amateur experimenters, and to render the "Annals" still more generally useful than heretofore, we are in hopes that, by introducing this novel feature into the work, much may be accomplished by assisting those of our readers who, in consequence of the defective condition of our old standard works, and the imperfections and palpable absurdities which later writers have introduced to their productions on these subjects, may be without any other guide in conducting their experimental inquiries.

We are well aware of "the lamentable defect in this kind of knowledge which has recently been elicited in a quarter where one would least have expected it," as observed by a correspondent. And, "probably a few close discussions might be the means of developing the talents of the *élite* of British electricians: for, although silence may possibly be a wisdom in some of them who have had fair opportunities of exercising their puissance in support of their pretended discoveries, their declining to enter the lists is no favourable interpretation of their claims to public credence."

LECTURE I.

In introducing these lectures to the readers of the "Annals of Electricity, Magnetism, and Chemistry, &c.," as they are intended principally for the instruction of amateur experimenters, it will be necessary to avoid, as much as possible, all those phrases and technicalities which not only puzzle, but absolutely mislead, even those who have, in their own estimation, much higher pretensions to a knowledge of such fashionable appendages to scientific literature, than the persons for whose instruction these lectures are intended; and to whom, therefore, I shall address myself with freedom, and in the plainest language that the present state of these subjects appear to me to be capable of admitting. I do not, however, wish to enter into any engagement that would limit my labours to the humble task of a mere detail of facts, without linking them together in some theoretical system or systems of physical laws; because one of my objects is to trace to the same operations of nature, those facts, and those only, which are easily, and not otherwise, explained, by that code of laws which governs the display of one peculiar class of phenomena. And not to encumber any theoretical system with those phenomena to which they do not appear to belong: but to explain each class of phenomena by its own peculiar code of laws; or if you please, by its own peculiar theory. Hence it is that I shall be expected to be explicit on every point on which I touch, both experimental and theoretical, and either undertake to explain all those experimental facts which I may consider necessary to bring forward, or candidly acknowledge that they are inexplicable upon the theoretical principles which I advance.

It will here be necessary to enter into certain conditions with my readers, respecting some of those theoretical points, which to many philosophers, even of the present day, appears to be somewhat doubtful: though I believe the opinions of many others are

favourable to those theoretical views by which I propose to be guided.

I wish to be understood then, before I proceed any farther, that, besides those recognised portions of matter which appear to be the principal part of the materials which constitute the earth and its atmosphere, such as the various kinds of solids and fluids which usually receive these general appellations, there are, at least, *three* others, whose reciprocal actions on each other, and whose peculiar operations on the former classes of bodies, are productive of the most surprising, and, in our present state of physical knowledge, the most interesting phenomena that nature has revealed to man. These are the *electric matter*;—the *magnetic matter*;—and the *calorific matter*; each of which I shall consider as a distinct element, possessing peculiarities of force and modes of action, and exhibiting phenomena which no other kind of matter has the power of displaying. They, however, operate on one another in a very remarkable manner, by their peculiar reciprocal excitations, and are thus productive of phenomena which have led some philosophers to the belief of their complete identity.

The fineness and subtlety of the *electric*, the *magnetic*, and the *calorific* particles, lead us to infer that they insinuate themselves into the pores of all other kinds of terrestrial matter; and their inactivity, when unmolested in these their natural habitations, is obviously a consequence of the equilibriums of their respective forces, when in an undisturbed state. So long, therefore, as these natural equilibriums remain unmolested, all of these material agents are perfectly inactive and exhibit no phenomenon whatever. Hence it is, that some exciting process becomes absolutely necessary before any of their respective phenomena can be produced. The processes of excitation which may be employed for bringing these agencies into a state of activity are exceedingly various, as I shall have occasion to show in many parts of these lectures; but for the present, it will be sufficient that I describe one simple mode only, of exciting each individual agent, by means of which, certain phenomena of each class may very easily be brought to pass.

If you take a stick of sealing wax, and, without any preparation, present it to any very light article, such as small feathers, bits of thin paper, &c. placed either on a table, book, or a dish, &c., you will not perceive any action whatever exercised by the wax on these light bodies. In this case you may easily imagine, that there is a complete electrical equilibrium in the body and on the surface of the sealing wax; and also in the light articles to which it was presented: and that it is in consequence of this equilibrium that the electric matter is perfectly inert, and will not act upon the light bodies which you had prepared. I wish it to be understood, however, that, although the electric forces of the wax had not a sufficient degree of intensity to cause a disturbance in the light bodies, it is still possible, that there might not be an absolute uniformity in the distribution of the electric matter, either on the wax or on the other bodies.

Now warm the stick of sealing-wax, taking care not to heat it too much ; and then rub it on the sleeve of your coat.

By this simple process you have disturbed the previous electric equilibrium of the sealing-wax, and caused the electric forces to become sufficiently active to produce motion in the light bodies to which you now may present the stick. They will rise up and cling to the wax, often changing their positions on its surface, and sometimes they will be suddenly thrown off again to a neighbouring body, to which they will attach themselves for a short time, and again jump back again to the sealing-wax ; again leave it, and again return ; and so on for several times before the action ceases. These motions of the light bodies are electric phenomena ; and may be repeated many times by renewing the activity of the electric forces, by again rubbing the dry and warm sealing-wax on the sleeve of your coat.

If you prefer a piece of dry woollen cloth to the sleeve of your coat, you may rub the sealing-wax with it with the same effect. Or you may use a piece of dry and warm flannel to rub your wax against ; or the fur side of a hare-skin, or a rabbit-skin, which is, perhaps, better than any of the previously named substances. But whatever you may choose to rub the sealing-wax with, let me advise you to have it *warm* and *dry*, because much of your success in the experiment will depend on those conditions of both the *rubbing substance* and the sealing-wax.

It will now be proper to inform you that the motions which the light bodies make *towards* the sealing-wax are considered to be the effect of an electrical *attraction*, exerted between them and the wax ; and their motions *from* the wax are considered to be due to an electrical attraction exerted between them and the body to which they fly, and for a while attach themselves. Besides the force of electrical *attraction*, there is also a force of electrical *repulsion*, to which I shall solicit your attention more particularly in due course as we proceed.

There are many other bodies which exhibit this class of electrical phenomena, by treating them in the manner I have described for sealing-wax. Such is the case with amber, sulphur, &c. If you use a glass tube for the exhibition of these electrical phenomena, it will also require to be warm and dry, not only on the outer surface, but on the inner surface also ; and the rubbing substance ought to be soft silk. A piece of old black silk answers as well as any thing. The rubbing process, in all these cases, whatever may be the nature of the articles employed, is called *excitation*.

When your sealing wax, or glass tube is well excited, and held at a short distance above the light bodies, the latter may be made to produce rapid motions to and fro, and dance on the table as if animated, by the active electric forces to which they are exposed. If you place your light bodies on a pewter or a silver plate, or on any metallic flat surface, their dancing motions will be more lively than when placed on any other kind of material : and if you touch

the metallic plate with one of your fingers, the activity of the motions will be considerably improved.

I will now solicit your attention to a simple mode of producing *magnetic* phenomena, which I consider to emanate from the energies of an agent perfectly distinct from the electric. You must allow me to suppose that you are already acquainted with an instrument called the magnetic needle; it is sometimes called the compass needle; and when supported on a finely pointed pivot, so as to rest on a horizontal plane, one of its ends, in these latitudes, points towards the north, inclining a little towards the west of that point; and its other extremity, consequently, points a little to the east of the true south. In many other parts on the earth's surface, the direction in which the magnetic needle places itself when at rest, relatively to the geographical meridian, is very different to that in which it reposes in this country. But in every part of the world it is subject to certain influences which are capable of communicating to it peculiar motions, and placing it stedfastly in other positions than those which it assumes when no such local influences are present.

If, after the magnetic needle has come to rest, you were to turn it on its pivot with your finger, so as to point to some other quarter of the world, and then take your finger away from it, the needle would commence a series of movements which would terminate by its settling again in its former position; showing that, by the operation of some hidden force or agency, the needle had a greater tendency to repose in one direction than in another; which, in England, and in many other countries, is more near to the meridian than to a line placed east and west; or to a circle of latitude at that place. With respect to the cause of this peculiar tendency of the needle to place itself in a north and south direction, I can only say, in this place, that it is so completely under the control of the magnetic forces of the earth, that they alone are supposed to constrain it to assume that particular direction; but why the earth is magnetic, and why its magnetic forces should be so situated as to operate on the needle in that peculiar manner, are matters which philosophers have not yet determined. There are, however certain laws of magnetic action which are well known, and which I will explain in a future lecture, my object at present, being that of showing the simplest, and most easily produced specimens of the three grand classes of phenomena which are so eminently conspicuous in nature, and so easily distinguished from each other.

Perhaps the simplest process for bringing the calorific matter from a state of inactive repose to a state of such activity as to produce ignition and fire, would be that of striking flint against hardened steel, and thus igniting detached particles of the metal, which in their turn, would ignite gunpowder, tinder, &c. In this case the calorific matter, which, previous to the collision of the flint and steel, was perfectly inactive, has, by the operation, become suddenly compressed into a smaller compass than that which it previously occupied, and becomes active fire in the condensed

state it is made to assume by the blow that is given to it by the flint and the steel. The blacksmith makes a nail red hot, by giving a few smart blows with a hammer; and the Indian obtains fire by rubbing two blocks of wood against each other. These, and many other mechanical processes, are productive of fire, by calling into action the calorific matter which, previously, was so perfectly inert, as to be incapable of igniting the most inflammable matter. In some chemical compounds this latent calorific matter is so susceptible of activity by mechanical operations, that it requires extreme caution to prevent their ignition, even during the necessary processes of preparing them, and transferring them from one vessel to another.

In the course of these lectures I shall have occasion to show that an *active* portion of the electric matter, has the power of disturbing an inactive portion, and thus causing it to become active also. Active portions of the electric matter will also disturb other active portions of it, and become productive of very interesting phenomena. Active magnetic matter is also productive of its own class of phenomena, by the operation of its peculiar forces on other portions of matter of its own kind: such, also, is the case with the calorific matter; for one portion will disturb another portion, and thus become the exciting cause for the display of other calorific phenomena. Moreover, these distinct kinds of matter have the power of reciprocally operating on one another, in such a manner as to become the existing agents for the display of each others phenomena. Hence, it is that we employ the terms *electro-magnetism*,—*magnetic-electricity*,—*thermo-electricity*, &c., the adjective in each expression implying the exciting agent, and the noun the character of the phenomena produced. I shall also have to employ the terms *galvanic-electricity* and *voltic-electricity*, all of which terms I shall endeavour to explain in their proper places as I proceed.

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W. H. C.

W. H. C.







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FOURTEENTH SERIES.

LII.—§ 20. *Nature of the electric force or forces* § 21. *Relation of the electric and magnetic forces.* § 22. *Note on electrical excitation.*

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§ 20. *Nature of the electric force or forces.*

1667. The theory of induction set forth and illustrated in the three preceding series of experimental researches does not assume anything new as to the nature of the electric force or forces, but only as to their distribution. The effects may depend upon the association of one electric fluid with the particles of matter, as in the theory of Franklin, Epinus, Cavendish, and Mossotti; or they may depend upon the association of two electric fluids, as in the theory of Dufay and Poisson; or they may not depend upon anything which can properly be called the electric fluid, but on vibrations or other affections of the matter in which they appear. The theory is unaffected by such differences in the mode of viewing the nature of the forces; and though it professes to perform the important office of stating *how* the powers are arranged (at least in inductive phenomena), it does not, as far as I can yet perceive, supply a single experiment which can be considered as a distinguishing test of the truth of any one of these various views.

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1668. But, to ascertain how the forces are arranged, to trace them in their various relations to the particles of matter, to determine their general laws, and also the specific differences which occur under these laws, is as important as, if not more so than, to know whether the forces reside in a fluid or not; and with the hope of assisting in this research, I shall offer some further developments, theoretical and experimental, of the conditions under which I suppose the particles of matter are placed when exhibiting inductive phenomena.

1669. The theory assumes that all the *particles*, whether of insulating or conducting matter, are as wholes conductors.

1670. That not being polar in their normal state, they can become so by the influence of neighbouring charged particles, the polar state being developed at the instant, exactly as in an insulated conducting *mass* consisting of many particles.

1671. That the particles when polarized are in a forced state, and tend to return to their normal or natural condition.

1672. That being as wholes conductors, they can readily be charged, either *bodily* or *polarly*.

1673. That particles which being contiguous are also in the line of inductive action can communicate or transfer their polar forces one to another *more or less* readily.

1674. That those doing so less readily require the polar forces to be raised to a higher degree before this transference or communication takes place.

1675. That the *ready* communication of forces between contiguous particles constitutes *conduction*, and the *difficult* communication *insulation*; conductors and insulators being bodies whose particles naturally possess the property of communicating their respective forces easily or with difficulty; having these differences just as they have differences of any other natural property.

1676. That ordinary induction is the effect resulting from the action of matter charged with excited or free electricity upon insulating matter, tending to produce in it an equal amount of the contrary state.

1677. That it can do this only by polarizing the particles contiguous to it, which perform the same office to the next, and these again to those beyond; and that thus the action is propagated from the excited body to the next conducting mass, and there renders the contrary force evident in consequence of the effect of communication which supervenes in the conducting mass upon the polarization of the particles of that body (1675.).

1678. That therefore induction can only take place through or across insulators; that induction is insulation, it being the neces-

sary consequence of the state of the particles and the mode in which the influence of electrical forces is transferred or transmitted through or across such insulating media.

1679. The particles of an insulating dielectric whilst under induction may be compared to a series of small magnetic needles, or more correctly still to a series of small insulated conductors. If the space round a charged globe were filled with a mixture of an insulating dielectric, as oil of turpentine or air, and small globular conductors, as shot, the latter being at a little distance from each other so as to be insulated, then these would in their condition and action exactly resemble what I consider to be the condition and action of the particles of the insulating dielectric itself (1837). If the globe were charged, these little conductors would all be polar; if the globe were discharged, they would all return to their normal state, to be polarized again upon the recharging of the globe. The state developed by induction through such particles on a mass of conducting matter at a distance would be of the contrary kind, and exactly equal in amount to the force in the inductive globe. There would be a lateral diffusion of force (1224. 1297.), because each polarized sphere would be in an active or tense relation to all those contiguous to it, just as one magnet can affect two or more magnetic needles near it, and these again a still greater number beyond them. Hence would result the production of curved lines of inductive force if the inductive body in such a mixed dielectric were an uninsulated metallic ball (1219, &c.) or other properly shaped mass. Such curved lines are the consequences of the two electric forces arranged as I have assumed them to be: and, that the inductive force can be directed in such curved lines is the strongest proof of the presence of the two powers and the polar condition of the dielectric particles.

1680. I think it is evident, that in the case stated, action at a distance can only result through an action of the contiguous conducting particles. There is no reason why the inductive body should polarize or affect *distant* conductors and leave those *near* it, namely the particles of the dielectric, unaffected: and everything in the form of fact and experiment with conducting masses or particles of a sensible size contradicts such a supposition.

1681. A striking character of the electric power is that it is limited and exclusive, and that the two forces being always present are exactly equal in amount. The forces are related in one of two ways, either as in the natural normal condition of an uncharged insulated conductor; or as in the charged state, the latter being a case of induction.

1682. Cases of induction are easily arranged so that the two forces being limited in their direction shall present no phenomena or indications external to the apparatus employed. Thus, if a Leyden jar, having its external coating a little higher than the in-

ternal, be charged and then its charging ball and rod removed, such jar will present no electrical appearances so long as its outside is uninsulated. The two forces which may be said to be in the coatings, or in the particles of the dielectric contiguous to them, are entirely engaged to each other by induction through the glass; and a carrier ball (1181.) applied either to the inside or outside of the jar will show no signs of electricity. But if the jar be insulated, and the charging ball and rod, in an uncharged state and suspended by an insulating thread of white silk, be restored to their place, then the part projecting above the jar will give electrical indications and charge the carrier, and at the same time the *outside* coating of the jar will be found in the opposite state and inductive towards external surrounding objects.

1683. These are simple consequences of the theory. Whilst the charge of the inner coating could induce only through the glass towards the outer coating, and the latter contained no more of the contrary force than was equivalent to it, no induction external to the jar could be perceived; but when the inner coating was extended by the rod and ball so that it could induce through the air towards external objects, then the tension of the polarized glass molecules would, by their tendency to return to the normal state, fall a little, and a portion of the charge passing to the surface of this new part of the inner conductor, would produce inductive action through the air towards distant objects, whilst at the same time a part of the force in the outer coating previously directed inwards would now be at liberty, and indeed be constrained to induct outwards through the air, producing in that outer coating what is sometimes called, though I think very improperly, free charge. If a small Leyden jar be converted into that form of apparatus usually known by the name of the electric well, it will illustrate this action very completely.

1684. The terms *free charge* and *dissimulated electricity* convey therefore erroneous notions if they are meant to imply any difference as to the mode or kind of action. The charge upon an insulated conductor in the middle of a room is in the same relation to the walls of that room as the charge upon the inner coating of a Leyden jar is to the outer coating of the same jar. The one is not more *free* or more *dissimulated* than the other; and when sometimes we make electricity appear where it was not evident before, as upon the outside of a charged jar, when, after insulating it, we touch the inner coating, it is only because we divert more or less of the inductive force from one direction into another; for not the slightest change is in such circumstances impressed upon the character or action of the force.

1685. Having given this general theoretical view, I will now notice particular points relating to the nature of the assumed electric polarity of the insulating dielectric particles.

1686. The polar state may be considered in common induction as a forced state, the particles tending to return to their normal condition. It may probably be raised to a very high degree by approximation of the inductive and inductuous bodies or by other circumstances; and the phenomena of electrolyzation (861. 1652. 1706.) seem to imply that the quantity of power which can thus be accumulated on a single particle is enormous. Hereafter we may be able to compare corpuscular forces, as those of gravity, cohesion, electricity, and chemical affinity, and in some way or other from their effects deduce their relative equivalents; at present we are not able to do so, but there seems no reason to doubt that their electrical, which are at the same time their chemical forces (891. 918.), will be by far the most energetic.

1687. I do not consider the powers when developed by the polarization as limited to two distinct points or spots on the surface of each particle to be considered as the poles of an axis, but as resident on large portions of that surface, as they are upon the surface of a conductor of sensible size when it is thrown into a polar state. But it is very probable, notwithstanding, that the particles of different bodies may present specific differences in this respect, the powers not being equally diffused though equal in quantity; other circumstances also, as form and quality, giving to each a peculiar polar relation. It is perhaps to the existence of some such differences as these that we may attribute the specific actions of the different dielectrics in relation to discharge (1394. 1508.) Thus with respect to oxygen and nitrogen singular contrasts were presented when spark and brush discharge were made to take place in these gases, as may be seen by reference to the Table in paragraph 1518 of the Thirteenth Series; for with nitrogen, when the small negative or the large positive ball was rendered inductive, the effects corresponded with those which in oxygen were produced when the small positive or the large negative ball was rendered inductive.

1688. In such solid bodies as glass, lac, sulphur, &c., the particles appear to be able to become polarized in all directions, for a mass when experimented upon so as to ascertain its inductive capacity in three or more directions (1690.), gives no indication of a difference. Now as the particles are fixed in the mass, and as the direction of the induction through them must change with its change relative to the mass, the constant effect indicates that they can be polarized electrically in any direction. This accords with the view already taken of each particle as a whole being a conductor (1669.), and, as an experimental fact, helps to confirm that view.

1689. But though particles may thus be polarized in any direction under the influence of powers which are probably of extreme energy (1686.), it does not follow that each particle may not tend to polarize to a greater degree, or with more facility, in one direc-

tion than another; or that different kinds may not have specific differences in this respect, as they have differences of conducting and other powers (1296. 1326. 1395). I sought with great anxiety for a relation of this nature; and selecting crystalline bodies as those in which all the particles are symmetrically placed, and therefore best fitted to indicate any result which might depend upon variation of the direction of the forces to the direction of the particles in which they were developed, experimented very carefully with them. I was the more strongly stimulated to this inquiry by the beautiful electrical condition of the crystalline bodies tourmaline and boracite, and hoped also to discover a relation between electric polarity and that of crystallization, or even of cohesion itself (1316.). My experiments have not established any connexion of the kind sought for. But as I think it of equal importance to show either that there is or is not such a relation, I shall briefly describe the results.

1690. The form of experiment was as follows. A brass ball 0.73 of an inch in diameter, fixed at the end of a horizontal brass rod, and that at the end of a brass cylinder, was by means of the latter connected with a large Leyden battery (291.) by perfect metallic communications, the object being to keep that ball, by its connexion with the charged battery in an electrified state, very nearly uniform, for half an hour at a time. This was the inductric ball. The inducteous ball was the carrier of the torsion electrometer (1229. 1314); and the dielectric between them was a cube cut from a crystal, so that two of its faces should be perpendicular to the optical axis, whilst the other four were parallel to it. A small projecting piece of shell-lac was fixed on the inductric ball at that part opposite to the attachment of the brass rod, for the purpose of preventing actual contact between the ball and the crystal cube. A coat of shell-lac was also attached to that side of the carrier ball which was to be towards the cube, being also that side which was furthest from the repelled ball in the electrometer when placed in its position in that instrument. The cube was covered with a thin coat of shell-lac dissolved in alcohol, to prevent the deposition of damp upon its surface from the air. It was supported upon a small table of shell-lac fixed on the top of a stem of the same substance, the latter being of sufficient strength to sustain the cube, and yet flexible enough from its length to act as a spring, and allow the cube to bear, when in its place, against the shell-lac on the inductric ball.

1691. Thus it was easy to bring the inducteous ball always to the same distance from the inductric ball, and to uninsulate and insulate it again in its place; and then, after measuring the force in the electrometer (1181.), to return it to its place opposite to the inductric ball for a second observation. Or it was easy by revolving the stand which supported the cube to bring four of its faces in succession towards the inductric ball, and so observe the force

when the lines of inductive action (1804.) coincided with, or were transverse to, the direction of the optical axis of the crystal. Generally from twenty to twenty-eight observations were made in succession upon the four vertical faces of a cube, and then an average expression of the inductive force was obtained, and compared with similar averages obtained at other times, every precaution being taken to secure accurate results.

1692. The first cube used was of *rock crystal*; it was 0.7 of an inch in the side. It presented a remarkable and constant difference, the average of not less than 197 observations, giving 100 for the specific inductive capacity in the direction coinciding with the optical axis of the cube, whilst 93.59 and 93.31 were the expressions for the two transverse directions.

1693. But with a second cube of rock crystal corresponding results were not obtained. It was 0.77 of an inch in the side. The average of many experiments gave 100 for the specific inductive capacity coinciding with the direction of the optical axis, and 98.6 and 99.92 for the two other directions.

1694. Lord Ashley, whom I have found ever ready to advance the cause of science, obtained for me the loan of three globes of rock crystal belonging to Her Grace the Duchess of Sutherland for the purposes of this investigation. Two had such fissures as to render them unfit for the experiments (1193. 1698.). The third, which was very superior, gave me no indications of any difference in the inductive force for different directions.

1695. I then used cubes of Iceland spar. One 0.5 of an inch in diameter gave 100 for the axial direction, and 98.66 and 95.74 for the two cross directions. The other, 0.8 of an inch in the side, gave 100 for the axial direction, whilst 101.73 and 101.86 were the numbers for the cross direction.

1696. Besides these differences there were others, which I do not think it needful to state, since the main point is not confirmed. For though the experiments with the first cube raised great expectation, they have not been generalized by those which followed. I have no doubt of the results as to that cube, but they cannot as yet be referred to crystallization. There are in the cube some faintly coloured layers parallel to the optical axis, and the matter which colours them may have an influence; but then the layers are also nearly parallel to a cross direction, and if at all influential should show some effect in that direction also, which they did not.

1697. In some of the experiments one half or one part of a cube showed a superiority to another part, and this I could not trace to any charge the different parts had received. It was found that the varnishing of the cubes prevented any communication of charge to them, except (in a few experiments) a small degree of the negative state, or that which was contrary to the state of the inductric ball (1564. 1566.).

1698. I think it right to say that, as far as I could perceive, the insulating character of the cubes used was perfect, or at least so nearly perfect, as to bear a comparison with shell-lac, glass, &c. (1255.). As to the cause of the differences, other than regular crystalline structure, there may be several. Thus minute fissures in the crystal insensible to the eye may be so disposed as to produce a sensible electrical difference (1193.). Or the crystallization may be irregular; or the substance may not be quite pure; and if we consider how minute a quantity of matter will alter greatly the conducting power of water, it will seem not unlikely that a little extraneous matter diffused through the whole or part of a cube, may produce effects sufficient to account for all the irregularities of action that have been observed.

1699. An important inquiry regarding the electrical polarity of the particles of an insulating dielectric, is, whether it be the molecules of the particular substance acted on, or the component or ultimate particles, which thus act the part of insulated conducting polarizing portions (1669.).

1700. The conclusion I have arrived at is, that it is the molecules of the substance which polarize as wholes (1847.); and that however complicated the composition of a body may be, all those particles or atoms which are held together by chemical affinity to form one molecule of the resulting body, act as one conducting mass or particle when inductive phenomena and polarization are produced in the substance of which it is a part.

1701. This conclusion is founded on several considerations. Thus if we observe the insulating and conducting power of elements when they are used as dielectrics, we find some, as sulphur, phosphorous, chlorine, iodine, &c., whose particles insulate, and therefore polarize in a high degree; whereas others, as the metals, give scarcely any indication of possessing a sensible proportion of this power (1828.), their particles freely conducting one to another. Yet when these enter into combination they form substances having no direct relation apparently, in this respect, to their elements; for water, sulphuric acid, and such compounds formed of insulating elements, conduct by comparison freely; whilst oxide of lead, flint glass, borate of lead, and other metallic compounds containing very high proportions of conducting matter, insulate excellently well. Taking oxide of lead therefore as the illustration, I conceive that it is not the particles of oxygen and lead which polarize separately under the act of induction, but the molecules of oxide of lead which exhibit this effect, all the elements of one particle of the resulting body, being held together as parts of one conducting individual by the bonds of chemical affinity; which is but another term for electrical force (918.).

1702. In bodies which are electrolytes we have still further reason for believing in such a state of things. Thus when water,

chloride of tin, iodide of lead, &c. in the solid state are between the electrodes of the voltaic battery, their particles polarize as those of any other insulating dielectric do (1164.); but when the liquid state is conferred on these substances, the polarized particles divide, the two halves, each in a highly charged state, travelling onwards until they meet other particles in an opposite and equally charged state, with which they combine, to the neutralization of their chemical, i. e. their electrical forces, and the reproduction of compound particles, which can again polarize as wholes, and again divide to repeat the same series of actions (1347.).

1703. But though electrolytic particles polarize as wholes, it would appear very evident that in them it is not a matter of entire indifference *how* the particle polarizes (1689.), since, when free to move (380, &c.) the polarities are ultimately distributed in reference to the elements; and sums of force equivalent to the polarities, and very definite in kind and amount, separate, as it were, from each other, and travel onwards with the elementary particles. And though I do not pretend to know what an atom is, or how it is associated or endowed with electrical force, or how this force is arranged in the cases of combination and decomposition, yet the strong belief I have in the electrical polarity of particles when under inductive action, and the bearing of such an opinion on the general effects of induction, whether ordinary or electrolytic, will be my excuse, I trust, for a few hypothetical considerations.

1704. In electrolyzation it appears that the polarized particles would (because of the gradual change which has been induced upon the chemical, i. e. the electrical forces of their elements (918.)) rather divide than discharge to each other without division (1348.); for if their division, i. e. their decomposition and recombination, be prevented by giving them the solid state, then they will insulate electricity perhaps a hundredfold more intense than that necessary for their electrolyzation (419, &c.). Hence the tension necessary for direct conduction in such bodies appears to be much higher than that for decomposition (+19. 1164. 1344.).

1705. The remarkable stoppage of electrolytic conduction by solidification (380. 1358.), is quite consistent with these views of the dependence of that process on the polarity which is common to all insulating matter when under induction, though attended by such peculiar electro-chemical results in the case of electrolytes. Thus it may be expected that the first effect of induction is so to polarize and arrange the particles of water that the positive or hydrogen pole of each shall be from the positive electrode and towards the negative electrode, whilst the negative or oxygen pole of each shall be in the contrary direction; and thus when the oxygen and hydrogen of a particle of water have separated, passing to and combining with other hydrogen and oxygen particles, unless these new particles of water could turn round they could not take up that position necessary for their successful electrolytic polariza-

tion. Now solidification, by fixing the water particles and preventing them from assuming that essential preliminary position, prevents also their electrolysis (413.); and so the transfer of forces in that manner being prevented (1347. 1703.), the substance acts as an ordinary insulating dielectric (for it is evident by former experiments (419. 1704.) that the insulating tension is higher than the electrolytic tension), induction through it rises to a higher degree, and the polar condition of the molecules as wholes, though greatly exalted, is still securely maintained.

1706. When decomposition happens in a fluid electrolyte, I do not suppose that all the molecules in the same sectional plane (1634.) part with and transfer their electrified particles or elements at once. Probably the *discharge force* for that plane is summed up on one or a few particles, which decomposing, travelling and recombining, restore the balance of forces, much as in the case of spark disruptive discharge (1406.); for as those molecules resulting from particles which have just transferred power must by their position (1705.) be less favourably circumstanced than others, so there must be some which are most favourably disposed, and these, by giving way first, will for the time lower the tension and produce discharge.

1707. In former investigations of the action of electricity (821, &c.) it was shown, from many satisfactory cases, that the quantity of electric power transferred onwards was in proportion to and was definite for a given quantity of matter moving as anion or cation onwards in the electrolytic line of action; and there was strong reason to believe that each of the particles of matter then dealt with, had associated with it a definite amount of electrical force, constituting its force of chemical affinity, the chemical equivalents and the electro-chemical equivalents being the same (836.). It was also found with few, and I may now perhaps say with no exceptions (1341.), that only those compounds containing elements in single proportions could exhibit the characters and phenomena of electrolytes (697.); oxides, chlorides, and other bodies containing more than one proportion of the electro negative element refusing to decompose under the influence of the electric current.

1708. Probable reasons for these conditions and limitations arise out of the molecular theory of induction. Thus when a liquid dielectric, as chloride of tin, consists of molecules, each composed of a single particle of each of the elements, then as these can convey equivalent opposite forces by their separation in opposite directions, both decomposition and transfer can result. But when the molecules, as in the bichloride of tin, consist of one particle or atom of one element, and two of the other, then the simplicity with which the particles may be supposed to be arranged and to act, is destroyed. And, though it may be conceived that when the molecules of bichloride of tin are polarized as wholes by the induction across them, the positive polar force might accumulate on

the one particle of tin whilst the negative polar force accumulated on the two particles of chlorine associated with it, and that these might respectively travel right and left to unite with other two of chlorine and one of tin, in analogy with what happens in cases of compounds consisting of single proportions, yet this is not altogether so evident or probable. For when a particle of tin combines with two of chlorine, it is difficult to conceive that there should not be some relation of the three in the resulting molecule analogous to fixed position, the one particle of metal being perhaps symmetrically placed in relation to the two of chlorine: and, it is not difficult to conceive of such particles that they could not assume that position dependent both on their polarity and the relation of their elements, which appears to be the first step in the process of electrolyzation (1345. 1705.).

§. 21. *Relation of the electric and magnetic forces.*

1709. I have already ventured a few speculations respecting the probable relation of magnetism, as the transverse force of the current, to the divergent or transverse force of the lines of inductive action belonging to static electricity (1658, &c.).

1710. In the further consideration of this subject it appeared to me to be of the utmost importance to ascertain, if possible, whether this lateral action which we call magnetism, or sometimes the induction of electrical currents (26. 1048, &c.), is extended to a distance *by the action of the intermediate particles* in analogy with the induction of static electricity, or the various effects, such as conduction, discharge, &c., which are dependent on that induction; or, whether its influence at a distance is altogether independent of such intermediate particles (1662.).

1711. I arranged two magneto-electric helices with iron cores end to end, but with an interval of an inch and three quarters between them, in which interval was placed the end or po.e of a bar magnet. It is evident, that on moving the magnetic pole from one core towards the other, a current would tend to form in both helices, in the one because of the lowering, and in the other because of the strengthening of the magnetism induced in the respective soft iron cores. The helices were connected together, and also with a galvanometer, so that these two currents should coincide in direction, and tend by their joint force to deflect the needle of the instrument. The whole arrangement was so effective and delicate, that moving the magnetic pole about the eighth of an inch to and fro two or three times, in periods equal to those required for the vibrations of the galvanometer needle, was sufficient to cause considerable vibration in the latter; thus showing readily the consequence of strengthening the influence of the magnet on the one core and helix, and diminishing it on the other.

1712. Then without disturbing the distances of the magnet and

cores, plates of substances were interposed. Thus calling the two cores A and B, a plate of shell-lac was introduced between the magnetic pole and A for the time occupied by the needle in swinging one way ; then it was withdrawn for the time occupied in the return swing ; introduced again for another equal portion of time ; withdrawn for another portion, and so on eight or nine times ; but not the least effect was observed on the needle. In other cases the plate was alternated, i. e. it was introduced between the magnet and A for one period of time, withdrawn and introduced between the magnet and B for the second period, withdrawn and restored to its first place for the third period, and so on, but with no effect on the needle.

1713. In these experiments *shell-lac* in plates 0·9 of an inch in thickness, *sulphur* in a plate 0·9 of an inch in thickness, and *copper* in a plate 0·7 of an inch in thickness were used without any effect. And I conclude that bodies, contrasted by the extremes of conducting and insulating power, and opposed to each other as strongly as metals, air, and sulphur, show no difference with respect to magnetic forces when placed in their lines of action, at least under the circumstances described.

1714. With a plate of iron, or even a small piece of that metal, as the head of a nail, a very different effect was produced, for then the galvanometer immediately showed its sensibility, and the perfection of the general arrangement.

1715. I arranged matters so that a plate of *copper* 0·2 of an inch in thickness, and ten inches in diameter, should have the part near the edge interposed between the magnet and the core, in which situation it was first rotated rapidly, and then held quiescent alternately, for periods according with that required for the swinging of the needle ; but not the least effect upon the galvanometer was produced.

1716. A plate of shell-lac 0·6 of an inch in thickness was applied in the same manner, but whether rotating or not it produced no effect.

1717. Occasionally the plane of rotation was directly across the magnetic curve : at other times it was made as oblique as possible ; the direction of the rotation being also changed in different experiments, but not the least effect was produced.

1718. I now removed the helices with their soft iron cores, and replaced them by two *flat helices* wound upon card board, each containing forty-two feet of silked copper wire, and having no associated iron. Otherwise the arrangement was as before, and exceedingly sensible ; for a very slight motion of the magnet between the helices produced an abundant vibration of the galvanometer needle.

1719. The introduction of plates of shell-lac, sulphur, or copper into the intervals between the magnet and these helices (1713.), produced not the least effect, whether the former were quiescent or

in rapid revolution (1715.). So here no evidence of the influence of the intermediate particles could be obtained (1710.).

1720. The magnet was then removed and replaced by a flat helix, corresponding to the two former, the three being parallel to each other. The middle helix was so arranged that a voltaic current could be sent through it at pleasure. The former galvanometer was removed, and one with a double coil employed, one of the lateral helices being connected with one coil, and the other helix with the other coil, in such manner that when a voltaic current was sent through the middle helix its inductive action (26.) on the lateral helices should cause currents in them, having contrary directions in the coils of the galvanometer. By a little adjustment of the distances these induced currents were rendered exactly equal, and the galvanometer needle remained stationary notwithstanding their frequent production in the instrument. I will call the middle coil C, and the external coils A and B.

1721. A plate of copper 0·7 of an inch thick and six inches square, was placed between coils C and B, their respective distances remaining unchanged; and then a voltaic current from twenty pairs of 4-inch plates was sent through the coil C, and intermitted, in periods fitted to produce an effect on the galvanometer (1712.), if any difference had been produced in the effect of C on A and B. But notwithstanding the presence of air in one interval and copper in the other, the inductive effect was exactly alike on the two coils, and as if air had occupied both intervals. So that notwithstanding the facility with which any induced currents might form in the thick copper plate, the coil outside of it was just as much affected by the central helix C as if no such conductor as the copper had been there (65.).

1722. Then, for the copper plate was substituted one of sulphur 0·9 of an inch thick; still the results were exactly the same, i. e. there was no action at the galvanometer.

1723. Thus it appears that when a voltaic current in one wire is exerting its inductive action to produce a contrary or a similar current in a neighbouring wire, according as the primary current is commencing or ceasing, it makes not the least difference whether the intervening space is occupied by such insulating bodies as air, sulphur and shell-lac, or such conducting bodies as copper, and the other non-magnetic metals.

1724. A correspondent effect was obtained with the like forces when resident in a magnet thus. A single flat helix (1718.) was connected with a galvanometer, and a magnetic pole placed near to it; then by moving the magnet to and from the helix, or the helix to and from the magnet, currents were produced indicated by the galvanometer.

1725. The thick copper plate (1721.) was afterwards interposed between the magnetic pole and the helix; nevertheless on moving

these to and fro, effects, exactly the same in direction and amount, were obtained as if the copper had not been there. So also on introducing a plate of sulphur into the interval, not the least influence on the currents produced by motion of the magnet or coils could be obtained.

1726. These results, with many others which I have not thought it needful to describe, would lead to the conclusion that (judging by the *amount* of effect produced at a distance by forces transverse to the electric current, i. e. magnetic forces,) the intervening matter, and therefore the intervening particles, have nothing to do with the phenomena; or in other words, that though the inductive force of static electricity is transmitted to a distance by the action of the intermediate particles (1164. 1666.), the transverse inductive force of currents, which can also act at a distance, is not transmitted by the intermediate particles in a similar way.

1727. It is however very evident that such a conclusion cannot be considered as proved. Thus when the metal copper is between the pole and the helix (1715. 1719. 1725.) or between the two helices (1721.) we know that its particles are affected, and can by proper arrangements make their peculiar state for the time very evident by the production of either electrical or magnetical effects. It seems impossible to consider this effect on the particles of the intervening matter as independent of that produced by the inductric coil or magnet C, on the inducteous coil or core A (1715. 1721.); for since the inducteous body is equally affected by the inductric body whether these intervening and affected particles of copper are present or not (1723. 1725.), such a supposition would imply that the particles so affected had no reaction back on the original inductric forces. The more reasonable conclusion, as it appears to me, is, to consider these affected particles as efficient in continuing the action onwards from the inductric to the inducteous body, and by this very communication producing the effect of *no loss* of induced-power at the latter.

1728. But then it may be asked what is the relation of the particles of insulating bodies, such as air, sulphur, or lac. when *they* intervene in the line of magnetic action? The answer to this is at present merely conjectural. I have long thought there must be a particular condition of such bodies corresponding to the state which causes currents in metals and other conductors (26. 53. 191. 201. 213.); and considering that the bodies are insulators one would expect that state to be one of tension. I have by rotating non-conducting bodies near magnetic poles and poles near them, and also by causing powerful electric currents to be suddenly formed and to cease around and about insulators in various directions, endeavoured to make some such state sensible, but have not succeeded. Nevertheless, as any such state must be of exceedingly low intensity, because of the feeble intensity of the currents which are used to induce it, it may well be that the state may exist,

and may be discoverable by some more expert experimentalist, though I have not been able to make it sensible.

1729. It appears to me possible, therefore, and even probable that magnetic action may be communicated to a distance by the action of the intervening particles, in a manner having a relation to the way in which the inductive forces of static electricity are transferred to a distance (1677.); the intervening particles assuming for the time more or less of a peculiar condition, which (though with a very imperfect idea) I have several times expressed by the term *electro-tonic state* (60. 242. 1114. 1661.) I hope it will not be understood that I hold the settled opinion that such is the case. I would rather in fact have proved the contrary, namely, that magnetic forces are quite independent of the matter intervening between the inductric and the inducteous bodies; but I cannot get over the difficulty presented by such substances as copper, silver, lead, gold, carbon, and even aqueous solutions (201. 213.), which though they are known to assume a peculiar state whilst intervening between the bodies acting and acted upon (1727.), no more interfere with the final result than those which have as yet had no peculiarity of condition discovered in them.

1730. A remark important to the whole of this investigation ought to be made here. Although I think the galvanometer used as I have described it (1711. 1720.) is quite sufficient to prove that the final amount of action on each of the two coils or the two cores A and B (1713. 1719.) is equal, yet there is an effect which *may* be consequent on the difference of action of two interposed bodies which it would not show. As time enters as an element into these actions* (125.), it is very possible that the induced actions on the helices or cores A, B, though they rise to the same degree when air and copper, or air and lac are contrasted as intervening substances, do not do so in the same time; and yet, because of the length of time occupied by a vibration of the needle, this difference may not be visible, both effects rising to their maximum in periods so short as to make no sensible portion of that required for a vibration of the needle, and so exert no visible influence upon it.

1731. If the lateral or transverse force of electrical currents, or what appears to be the same thing, magnetic power, could be proved to be influential at a distance independently of the intervening contiguous particles, then, as it appears to me, a real distinction, of a high and important kind, would be established between the natures of these two forces (1654. 1664.). I do not mean that the powers are independent of each other and might be rendered separately active, on the contrary they are probably essentially associated (1654.), but it by no means follows that they

* See *Annales de Chimie*, 1833, tom. II. pp. 422, 428.

are of the same nature. In common statical induction, in conduction, and in electrolyzation, the forces at the opposite extremities of the particles which coincide with the lines of action, and have commonly been distinguished by the term electric, are polar, and in the cases of contiguous particles act only to insensible distances; whilst those which are transverse to the direction of these lines, and are called magnetic, are circumferential, act at a distance, and if not through the mediation of the intervening particles, have their relations to ordinary matter entirely unlike those of the electrical forces with which they are associated.

1732. To decide this question of the identity or distinction of the two kinds of power, and establish their true relation, would be exceedingly important. The question seems fully within the reach of experiment, and offers a high reward to him who will attempt its settlement.

1733. I have already expressed a hope of finding an effect or condition which shall be to statical electricity what magnetic force is to current electricity (1658.) If I could have proved to my own satisfaction that magnetic forces extended their influence to a distance by the conjoined action of the intervening particles in a manner analogous to that of electrical forces, then I should have thought that the lateral tension of the lines of inductive action (1659.), or that state so often hinted at as the electro-tonic state (1661. 1662.), was this related condition of statical electricity.

1734. It may be said that the state of *no lateral action* is to static or inductive force the equivalent of *magnetism* to current force; but that can only be upon the view that electric and magnetic action are, in their nature essentially different (1664.). If they are the same power, the whole difference in the results being the consequence of the difference of *direction*, then the normal or *undeveloped* state of electric force will correspond with the state of *no lateral action* of the magnetic state of the force; the electric current will correspond with the lateral effects commonly called magnetism: but the state of static induction which is between the normal condition and the current will still require a corresponding lateral condition in the magnetic series, presenting its own peculiar phenomena; for it can hardly be supposed that the normal electric, and the inductive or polarized electric, condition, can both have the same lateral relation. If magnetism be a separate and a higher relation of the powers developed, then perhaps the argument which presses for this third condition of that force would not be so strong.

1735. I cannot conclude these general remarks upon the relation of the electric and magnetic forces without expressing my surprise at the results obtained with the copper plate (1721. 1725.) The experiments with the flat helices represent one of the simplest cases of the induction of electrical currents (1720.); the effect, as is well known, consisting in the production of a momentary current in a wire at the instant when a current in the contrary direction be-

gins to pass through a neighbouring parallel wire, and the production of an equally brief current in the reverse direction when the determining current is stopped (26.). Such being the case, it seems very extraordinary that this induced current which takes place in the helix A when there is only air between A and C (1720.) should be equally strong when that air is replaced by an enormous mass of that excellently conducting metal copper (1721.). It might have been supposed that this mass would have allowed of the formation and discharge of almost any quantity of currents in it, which the helix C was competent to induce, and so in some degree have diminished if not altogether prevented the effect in A: instead of which, though we can hardly doubt that an infinity of currents are formed at the moment in the copper plate, still not the smallest diminution or alteration of the effect in A appears (65.). Almost the only way of reconciling this effect with generally received notions is, as it appears to me, to admit that magnetic action is communicated by the action of the intervening particles (1729. 1733.).

1736. This condition of things, which is very remarkable, accords perfectly with the effects observed in solid helices where wires are coiled over wires to the amount of five or six or more layers in succession, no diminution of effect on the outer ones being occasioned by those within.

§ 22. *Note on electrical excitation.*

1737. That the different modes in which electrical excitement takes place will some day or other be reduced under one common law can hardly be doubted, though for the present we are bound to admit distinctions. It will be a great point gained when these distinctions are, not removed, but understood.

1738. The strict relation of the electrical and chemical powers renders the chemical mode of excitement the most instructive of all, and the case of two isolated combining particles is probably the simplest that we possess. Here however the action is local, and we still want such a test of electricity as shall apply to it, to cases of current electricity, and also to those of static induction. Whenever by virtue of the previously combined condition of some of the acting particles (923.) we are enabled, as in the voltaic pile, to expand or convert the local action into a current, then chemical action can be traced through its variations to the production of *all* the phenomena of tension and the static state, these being in every respect the same as if the electric forces producing them had been developed by friction.

1739. It was Berzelius, I believe, who first spoke of the aptness of certain particles to assume opposite states when in presence of each other (959.). Hypothetically we may suppose these states to increase in intensity by increased approximation, or by heat, &c. until at a certain point combination occurs, accompanied by such an

arrangement of the forces of the two particles between themselves as is equivalent to a discharge, producing at the same time a particle which is throughout a conductor (1700.)

1740. This aptness to assume an excited electrical state (which is probably polar in those forming non-conducting matter) appears to be a primary fact, and to partake of the nature of induction (1162.), for the particles do not seem capable of retaining their particular state independently of each other (1177.) or of matter in the opposite state. What appears to be definite about the particles of matter is their assumption of a *particular* state, as the positive or negative, in relation to each other, and not of either one or other indifferently; and also the acquirement of force up to a certain amount.

1741. It is easily conceivable that the same force which causes local action between two free particles shall produce current force if one of the particles is previously in combination, forming part of an electrolyte (923. 1738.). Thus a particle of zinc, and one of oxygen, when in presence of each other, exert their inductive forces (1740.), and these at last rise up to the point of combination. If the oxygen be previously in union with hydrogen, it is held so combined by an analogous exertion and arrangement of the forces; and as the forces of the oxygen and hydrogen are for the time of combination mutually engaged and related, so when the superior relation of the forces between the oxygen and zinc come into play, the induction of the former or oxygen towards the metal cannot be brought on and increased without a corresponding deficiency in its induction towards the hydrogen with which it is in combination (for the amount of force in a particle is considered as definite), and the latter therefore has its force turned towards the oxygen of the next particle of water; thus the effect may be considered as extended to sensible distances, and thrown into the condition of static induction, which being discharged and then removed by the action of other particles produces currents.

1742. In the common voltaic battery, the current is occasioned by the tendency of the zinc to take the oxygen of the water from the hydrogen, the effective action being at the place where the oxygen leaves the previously existing electrolyte. But Schœnbein has arranged a battery in which the effective action is at the other extremity of this essential part of the arrangement, namely, where oxygen goes to the electrolyte.* The first may be considered as a case where the current is put into motion by the abstraction of oxygen from hydrogen, the latter by that of hydrogen from oxygen. The direction of the electric current is in both cases the same, when referred to the direction in which the elementary particles of the electrolyte are moving (923. 962.), and both are equally

* Philosophical Magazine, 1838, xii. 225, 315. See also De la Rive's results with peroxide of manganese. Annales de Chimie, 1836, lxi. p. 40.—*Dec.* 1838.

n accordance with the hypothetical view of the inductive action of the particles just described (1740.)

1743. In such a view of voltaic excitement, the action of the particles may be divided into two parts, that which occurs whilst the force in a particle of oxygen is rising towards a particle of zinc acting on it, and falling towards the particle of hydrogen with which it is associated (this being the progressive period of the inductive action), and that which occurs when the change of association takes place, and the particle of oxygen leaves the hydrogen and combines with the zinc. The former appears to be that which produces the current, or if there be no current, produces the state of tension at the termination of the battery; whilst the latter, by terminating for the time the influence of the particles which have been active, allows of others coming into play, and so the effect of current is continued.

1744. It seems highly probable, that excitement by friction may very frequently be of the same character. Wollaston endeavoured to refer such excitement to chemical action;* but if by chemical action ultimate union of the acting particles is intended, then there are plenty of cases which are opposed to such a view. Davy mentions some such, and for my own part I feel no difficulty in admitting other means of electrical excitement than chemical action, especially if by chemical action is meant a final combination of the particles.

1745. Davy refers experimentally to the opposite states which two particles having opposite chemical relations can assume when they are brought into the close vicinity of each other, but *not* allowed to combine†. This, I think, is the first part of the action already described (1743.); but in my opinion it cannot give rise to a continuous current unless combination takes place, so as to allow other particles to act successively in the same manner, and not even then unless one set of the particles be present as an element of an electrolyte (923. 963.); i. e. mere quiescent contact alone without chemical action does not in such cases produced a *current*.

1746. Still it seems very possible that such a relation may produce a high charge, and thus give rise to excitement by friction. When two bodies are rubbed together to produce electricity in the usual way, one at least must be an insulator. During the act of rubbing, the particles of opposite kinds must be brought more or less closely together, the few which are most favourably circumstanced being in such close contact as to be short only of that which is consequent upon chemical combination. At such moments they may acquire by their mutual induction (1740.) and partial discharge to each other, very exalted opposite states, and when, the moment after, they are by the progress of the rub removed from each other's

* Philosophical Transactions, 1801, p. 427.

† Ibid. 1807, p. 34.

vicinity, they will retain this state if both bodies be insulators, and exhibit them upon their complete separation.

1747. All the circumstances attending friction seems to me to favour such a view. The irregularities of form and pressure will cause that the particles of the two rubbing surfaces will be at very variable distances, only a few at once being in that very close relation which is probably necessary for the development of the forces; further, those which are nearest at one time will be further removed at another, and others will become the nearest, and so by continuing the friction many will in succession be excited. Finally, the lateral direction of the separation in rubbing seems to me the best fitted to bring many pairs of particles, first of all into that close vicinity necessary for their assuming the opposite states by relation to each other, and then to remove them from each other's influence whilst they retain that state.

1748. It would be easy, on the same view, to explain hypothetically, how, if one of the rubbing bodies be a conductor, as the amalgam of an electrical machine, the state of the other when it comes from under the friction is (as a mass) exalted; but it would be folly to go far into such speculation before that already advanced has been confirmed or corrected by fit experimental evidence. I do not wish it to be supposed that I think all excitement by friction is of this kind; on the contrary, certain experiments lead me to believe, that in many cases, and perhaps in all, effects of a thermo-electric nature conduce to the ultimate effect; and there are very probably other causes of electric disturbance influential at the same time, which we have not as yet distinguished.

*Royal Institution,
June, 1838.*

In a paper which was read at the Glasgow meeting of the "British Association for the Promotion of Science" I had occasion to trace the experiments of M. Schoenbein and others, on the inactivity of certain metals on acids, to others of a similar nature performed by Mr. Keir some fifty years ago. Since the reading of my paper at Glasgow, I have been requested to insert Mr. Keir's experiments in an early number of the "Annals," which I now do with great pleasure, as I think that many readers will be much interested by becoming acquainted with those original experiments of Keir, which, within the last few years, have commanded so much attention, as novelties emanating from the labours of other experimentors.

W. S.

LIII.—*Experiments and Observations on the Dissolution of Metals in Acids, and their Precipitations: with an Account of a New Compound Acid Menstrum useful in some mechanical operations of parting metals.* By JAMES KEIR, Esq., F.R.S. (Abridgement of the Philosophical Transaction of the Royal Society of London for the year 1790.)

In the following paper, says Mr. Keir, I intend to relate two sets of experiments: one, showing the effects of compounding the vitriolic and nitrous acids in dissolving metals: and the other, describing some curious appearances which occur in the precipitation of silver from its solution in nitrous acid by iron, and by some other substances. In a subsequent paper I hope to continue the subject of metallic dissolution* and precipitation, first, by adding some experiments on the quantities and kinds of gas produced by dissolving different metals in different acids, under various circumstances: 2ndly, by submitting certain general propositions, which seem deducible from the facts related; and lastly, by concluding with some reflections relative to the theory of metallic dissolution and precipitation.

PART 1.—*On the effects of Compounding the Vitriolic and Nitrous Acids, under various circumstances, on the dissolution of metals.*

§ 1.—*On the Mixture of Oil of Vitriol and Nitre.*—1. The properties of the several acids, in their separate states, have been investigated with considerable industry and success: and those of one compound, aqua regis, are well known, on account of its frequent use in dissolving gold: yet not only various other combinations of different acids remain to be examined; but also the changes of properties to which these mixed acids are subject, from the difference of circumstances, especially those of concentration, temperature, and of that quality which is called, properly or improperly, phlogistication, are subjects still open for inquiry.

2. As I shall have frequent occasion to speak of phlogistication and dephlogistication of acids, I wish to premise, that by these terms I mean only certain states or qualities of those bodies, but without any theoretic inference. Thus vitriolic acid may be said

* The English word solution has two significations in chemistry; one expressive of the act of dissolving, as when we say, that, "solution is a chemical operation;" and the other, denoting the substance dissolved in its solvent, as, "a solution of silver in nitrous acid." The French language is equally equivocal, as the word "dissolution" is used in both the above-mentioned senses. In treating on this subject, in which both meanings were very frequently required, sometimes in the same sentence, I could not but be sensible of confusion in the style, and I have therefore confined the word solution to express the substance dissolved together with its solvent, and the word dissolution to denote the act of dissolving.

to be phlogisticated by addition of sulphur or other inflammable matter, by which it is converted into sulphurous acid, without determining whether this change be caused by the addition of the supposed principle phlogiston, as one set of philosophers believe, or by the action of the added inflammable substance in drawing from the acid a portion of its aërial principle, by which the sulphur, its other element, is made to predominate, as others have lately maintained. It were much to be wished that we had words totally unconnected with theory; that chemists, who differ from each other in some speculative points, may yet speak the same language, and may relate their facts and observations, without having our attention continually drawn aside from these, to the different modes of explanation which have been imagined. But at present we have only the choice of terms between words derived from the ancient theory, and those which have been lately proposed by the opposers of that theory. In this dilemma I have preferred the use of the former, not that I wish to show any predilection to either theory, but because that system, having long been generally adopted, is understood by all parties: and principally because, by using the words of the old theory, I am at liberty to define them, and to give significations expressive merely of parts, and of the actual state of bodies; whereas the language and theory of the antiphlogistic chemists being interwoven and adapted to each other, the former cannot be divested of its theoretical reference, and therefore seems inapplicable to the mere exposition of facts, but ought to be reserved solely for the explanations of the doctrines from which this language is derived. Thus, by the definition before mentioned of phlogistication, this word expresses not the presence or existence of an hypothetical principle of inflammability, but a certain well known quality of acids and of other bodies, communicated to them by the addition of many actually inflammable substances. Thus, nitrous acid acquires a phlogisticated quality by addition of a little spirit of wine, or by distillation with any inflammable substance.

3. No two substances are more frequently in the hands of chemists and artists than vitriolic acid and nitre, yet I have found, that a mere mixture of these when much concentrated, possess properties which neither the vitriolic acid nor the nitrous, of the same degree of concentration, have, singly, and which could not be easily deduced, *a priori*, by reasoning from our present knowledge of the theory of chemistry.

4. Having found by some previous trials that a mixture composed of nitre dissolved in oil of vitriol was capable of dissolving silver easily and copiously, while it did not affect copper, iron, lead, regulus of cobalt, gold, platina, I conceived, that it might be useful in some cases of the parting of silver from copper and the other metals above mentioned; and having also observed, that the dissolving powers of the mixture of vitriolic and nitrous acids varied greatly in different degrees of concentration, and phlogistication, I thought that an in-

vestigation of these effects might be a subject fit for philosophical chemistry, and might tend to illustrate the theory of the dissolution of metals in acids. With these views I made the following experiments:—

5. I put into a long necked retort, the contents of which, including the neck, were 1400 grain measures, 100 grain measures of oil of vitriol of the usual density at which it is prepared in England, that is, whose specific gravity is to that of water as 1·844 to 1, and 100 grains of pure and clear nitre, which was then dissolved in the acid by the heat of a water-bath. To this mixture 100 grains of standard silver were added; the retort was set in a water-bath, in which the water was made to boil, and a pneumatic apparatus was applied to catch any air or gas which might be extricated.—The silver began to dissolve, and the solution became of a purple or violet colour, no air was thrown into the inverted jar, excepting a little of the common air of the retort, by means of the expansion which it suffered from the heat of the water-bath, and from some nitrous fumes which appeared in the retort, and which having afterwards condensed, occasioned the water to rise along the neck of the retort, and mix with the solution; the remaining silver was then separated and weighed, and it was found that 39 grains had been dissolved: but probably more would have been dissolved if the operation had not been interrupted by the water rushing into the retort.

6. In the same apparatus 200 grains of standard silver were added to a mixture of 100 grains of nitre, previously dissolved in 200 grain measures of oil of vitriol; and in this solvent, 92 grains of the silver were dissolved, without any production of air or gas. The solution, which was of a violet colour, having been poured out of the retort whilst warm (for with so a large a portion of nitre, such mixtures, especially after having dissolved silver, are apt to congeal with small degrees of cold), in order to separate the undissolved silver from it, and having been returned into the retort without this silver, I poured 200 grains of water into the retort, on which a strong effervescence took place between the solution and the water, and 3100 grain measures of nitrous gas were thrown into the inverted jar. On pouring 200 grains more of water into the retort, 600 grain measures of the same gas were expelled. Further additions of water yielded no more gas; neither did the silver, when afterwards added to this solution, give any sensible effervescence, or suffer a greater loss of weight than two grains.

7. In the same apparatus 100 grains of standard silver were exposed to a mixture of 80 grains of nitre dissolved in 200 grain measures of oil of vitriol; and in this operation 80 grains of silver were dissolved, while at the same time, 4500 grain measures of nitrous gas were thrown into the inverted jar. When the undissolved silver was removed, 200 grains of water were added to the solution, which was of a violet colour, and on the mixture of the

two fluids an effervescence happened ; but only a few bubbles of nitrous gas were then expelled.

8. In the same apparatus 100 grains of standard silver were exposed to a mixture of 200 grain measures of oil of vitriol, 200 grains of nitre, and 200 grains of water : and in this operation 20 grains of the silver were dissolved without any sensible emission of air or gas.

9. In these experiments, the copper contained in the standard silver gave a reddish colour to the saline mass which was formed in the solution, and seemed to be a calx of copper interspersed through the salt of silver. I perceived no other difference between the effects of pure and standard silver dissolved in this acid.

10. I then exposed tin to the same mixture of oil of vitriol and nitre, in the same apparatus, and in the same circumstances, taking care always to add more metal than could be dissolved, that, by weighing the remainder, the quantity capable of being dissolved might be found, as I had done with the experiments on silver ; and the results were as follow :—

11. No tin was dissolved nor calcined by the mixtures in the proportion of 200 grain measures of oil of vitriol to 200 grains of nitre : nor by any other mixture in the proportion of 200 grain measures of oil of vitriol to 150 grains of nitre, and consequently no gas was produced in either instance.

12. With a mixture in the proportion of 200 grain measures of oil of vitriol and 100 grains of nitre, the tin began soon to be acted on, and to be diffused through the liquor : but no extrication of gas appeared till the digestion had been continued two hours in boiling water ; and then it took place, and gave a frothy appearance to the mixture, which was of an opaque white colour, from the powder of tin being diffused among it. In this experiment, the quantity of tin thus calcined was 73 grains, and the quantity of nitrous gas extricated during this action on the tin, was 8500 grain measures. Then, on pouring 200 grains of water into the retort, a fresh effervescence took place between the water and the white opaque white mass, and 4600 grain measures of nitrous gas were thrown into the inverted receiver.

13. With a mixture in the proportion of 100 grain measures of oil of vitriol to 30 grains of nitre, 30 grains of tin were dissolved or calcined, and the nitrous gas, which began to be extricated much sooner than in the last mentioned experiment with a larger proportion of nitre, amounted to 6300 grain measures. Water, added to this solution of tin, did not produce any effervescence.

14. With a mixture in the proportion of 200 grain measures of oil of vitriol, 200 grains of nitre, and 200 grains of water, 133 grains of tin were acted on with an effervescence, which took place violently, and produced 6500 grain measures of nitrous gas.

15. The several mixtures above mentioned, in different proportions of nitre and oil of vitriol, did, by the help of the heat of the water-bath, calcined mercury into a white or grayish powder. Nicke was also partly calcined and partly dissolved by these mixtures. I did not perceive that any other metal was affected by them, excepting that the surfaces of some of them were tarnished.

16. These mixtures of oil of vitriol and nitre were apt to congeal by cold, those especially which had a large proportion of nitre thus, a mixture of 100 grain measures of oil of vitriol and 480 grains of nitre, after having kept fluid for several days, in a phial not so accurately stopped as to prevent altogether the escape of some white fumes, congealed at the temperature of 55° of Fahrenheit's thermometer: whereas some of the same liquid, having been mixed with equal parts of oil of vitriol, did not congeal with a less cold than 45° . The congelation is promoted by exposure to air, by which white fumes rise, and moisture may be absorbed, or by any other mode of slight dilution with water.

17. Dilution of this compound acid, with more or less water, alters considerably its properties, with regard to its action on metals. Thus it has been observed, that in its concentrated state it does not act on iron: but by adding water, it acquires a power of acting on that metal, and with different effect according to the proportion of the water added. Thus, by adding to two measures of the compound acid one measure of water, the liquor is rendered capable of calcining iron, and forming with it a white powder, but without effervescence. With an equal measure of water effervescence is produced. With a larger proportion of water the iron gave also a brown colour to the liquor, such as phlogisticated nitrous acid acquires from iron, or communicates to a solution of martial vitriol in water.

18. Dilution with water renders this compound acid capable of dissolving copper and zinc, and probably those other metals which are subject to the action of the dilute vitriolic or nitrous acid.

§ 2. *An account of a new process for separating silver from copper.*—19. The properties of this liquor, in dissolving silver easily, without acting on copper, have rendered it capable of a very useful application in the arts. Among the manufacturers at Birmingham, that of making vessels of silver plated on copper is a very considerable one. In cutting out the rolled plated metal into pieces of the required formes and sizes, there are many shreds, or scraps as they are called, unfit for any purpose but the recovery of the metals, by separating them from each other. The easiest and most economical method of parting these two metals, so as not to lose either of them, is an object of some consequence to the manufacturers. For this

purpose two modes were practised ; 1st, by melting the whole of the mixed metals with lead, and separating them by eliquation and testing ; & 2nd, by dissolving both metals in oil of vitriol, with the help of heat, and by separating the vitriol of copper, by dissolving it in water, from the vitriol of silver, which is afterwards to be reduced and purified. In the first of these methods, there is a considerable waste of lead and copper ; and in the second, the quantity of vitriolic acid employed is very great, as much more is dissipated in the form of volatile vitriolic, or sulphureous acid, than remains in the composition of the two vitriols.

Some years ago I communicated to an artist the method of affecting the separation of silver from copper by means of the above mentioned compound of vitriolic acid and nitre ; and, as I am informed, that it is now commonly practised by the manufacturers in Birmingham, I have no doubt but it is much more economical, and it is certainly much more easily executed, than any of the other methods ; for nothing more is required than to put the pieces of plated metal into an earthen-glazed pan, to pour on them some of the acid liquor, which may be in the proportion of 8 or 10 lb. of oil of vitriol to 1 lb. of nitre : to stir them about, that the surfaces may frequently be exposed to fresh liquor, and to assist the action by a gentle heat from 100° to 200° of Fahrenheit's scale. When the liquor is nearly saturated, the silver is to be precipitated from it by common salt, which forms a *luna cornea*, easily reducible by melting it in a crucible with a sufficient quantity of potash ; and lastly, by refining the melted silver, if necessary, with a little nitre thrown on it. In this manner the silver will be obtained sufficiently pure, and the copper will remain unchanged. Otherwise, the silver may be precipitated in its metallic state, by adding to the solution of silver a few pieces of copper, and a sufficient quantity of water to enable the liquor to act on the copper. The property which this acid mixture possesses of dissolving silver with great facility, and in considerable quantity, will probably render it a useful menstruum in the separation of silver from other metals ; and as the alchemists have distinguished the peculiar solvent of gold under the title of *aqua regis*, a name sufficiently distinctive, though founded on a fanciful allusion ; so, if they had been acquainted with the properties of this compound, they would probably have bestowed upon it the appellation of *aqua reginæ*.

§ 3.—*The change of properties communicated to the mixture of vitriolic and nitrous acids by phlogistigation.*—20. The above described compound acid may be phlogistigated by different methods, of which I shall mention three. First, By digesting the compound acid with sulphur by means of the heat of a water bath, the liquor dissolves the sulphur with effervescence, loses its property of yielding white fumes ; and if the quantity of sulphur be suffi-

cient, and if the heat applied be long enough continued, it exhibits red nitrous vapours, and assumes a violet colour.

Secondly—If, instead of dissolving nitre in concentrated vitriolic acid, this acid be impregnated with nitrous gas, or with nitrous vapour by making this gas, or vapour, pass into the acid, this compound will be phlogisticated, as it contains but only its phlogisticated part, not the entire nitrous acid, or element, the nitrous gas, without the proportion of pure air is necessary to constitute an acid. This impregnation of oil of vitriol with nitrous gas, or nitrous vapour, was first described, and some of the properties of the impregnated liquor noticed, by Dr. Priestly. (See Exp. and Obs. on Air, vol. 3, p. 129 and 217.) Thirdly, By substituting nitrous ammoniac instead of nitre in the mixture with oil of vitriol.

21. The compound prepared by any of these methods, but especially by the first and second, differs considerably in its properties with regard to its action on metals from the acid described in the first section. It has been observed, that the latter compound has little action on any metals but silver, tin, mercury, and nickel. On the other hand, the phlogisted compound not only acts on these, but also on several others. It forms with iron a beautiful rose-coloured solution, without application of any artificial heat: and in time a rose-coloured saline precipitate is deposited, which is soluble in water with considerable effervescence. It dissolves copper, and acquires from this metal, and also from regulus of cobalt, zinc, and lead, pretty deep violet tinges. Bismuth and regulus of antimony were also attacked by this phlogisticated acid. To ascertain more exactly the effects of this phlogisticated acid on some metals, I made the following experiments, with a liquor prepared by making nitrous gas pass through oil of vitriol during a considerable time.

22. To 200 grain measures of the oil of vitriol impregnated with nitrous gas, put into a retort with a long neck, the capacity of which, including the neck, was 1150 grain-measures, I added 144 grains of standard silver, and immersed the mouth of the retort in water, under an inverted jar filled with water, to catch the gas which might be extricated. The acid began to dissolve the silver without the application of heat; the solution became of a violet colour, and the quantity of nitrous gas received in the inverted jar was 14,700 grain measures. On weighing the silver remaining, the quantity which had been dissolved was found to be 70 grains. When water was added to the solution, an effervescence appeared, but only a very small quantity of gas was extricated. By means of water, a white saline powder of silver, soluble in a larger quantity of water, was precipitated from the solution. The solution of silver, when saturated and undiluted, congeals readily in cool temperature, and when diluted to a certain degree with water, gives foliated crystals.

23. In the same apparatus, and in the same manner, 100 grain

measures of this impregnated oil of vitriol were applied to iron. An effervescence appeared without the application of heat, the surface of the iron acquired a beautiful rose-colour or redness mixed with purple; and this colour gradually pervaded the whole liquor, but disappeared on keeping the retort some time in hot water. Notwithstanding a considerable apparent effervescence, the quantity of air expelled into the inverted jar was only 400 grain measures, of which one-fourth was nitrous, and the rest phlogisticated. The solution was then poured out of the retort, and the iron was found to have lost 2 grains in weight. The solution was returned into the retort without the iron, and 200 grains of water were added to it; on which a white powder was immediately precipitated, which re-dissolved with great effervescence. When 2000 grain measures of nitrous gas had been expelled into the inverted jar, without application of heat, the retort was placed in the water-bath, the heat of which rendered the effervescence so strong, that the liquor boiled over the neck of the retort, so that the quantity of gas extricated could not be ascertained.

24. In the same manner 11 grains of copper were dissolved in 100 grain measures of impregnated oil of vitriol. The solution was of a deep violet-colour, and at last was turbid. The quantity of nitrous gas expelled into the inverted jar during the operation was 4700 grain measures. When the copper was removed, and 200 grains of water were added to the solution, an effervescence took place, 1700 grain measures of nitrous gas were expelled, and the solution then acquired a blue-colour.

25. In the same apparatus and manner, 100 grain measures of the impregnated oil of vitriol were applied to tin, which was thence diminished in weight 16 grains, while the liquor acquired a violet-colour, became turbid by the suspension of the calx of tin, and a quantity of nitrous gas was thrown into the inverted receiver equal to 4100 grain measures, without application of heat, and another quantity equal to 4900 grain measures, after the retort was put into a water-bath.

26. Mercury added to the impregnated oil of vitriol formed a thick white turbid liquor, which was rendered clear by addition of unimpregnated oil of vitriol. In a little time this mixture continuing to act on the remaining mercury acquired a purple-colour. The mercury acted on, sunk to the bottom of the glass in the form of a white powder, and the purple liquor, when mixed with a solution of common salt in water, gave no appearance of it containing any mercury in a dissolved state.

27. The nitrous gas with which the oil of vitriol is impregnated shows no disposition to quit the acid by exposure to air; but, on adding water to the impregnated acid, the gas is expelled suddenly with great effervescence, and with red fumes, in consequence of its mixture with the atmospherical air. In adding 240 grains of water,

to 60 grain measures of impregnated oil of vitriol, 2300 grains of nitrous gas were thrown into the receiver ; but as the action of the two liquors is instantaneous, the quantity of gas expelled from the retort before its neck could be immersed in water, and placed under the receiver, must have been considerable. The whole of the gas, however, was not extricated by means of the water, for the remaining liquor dissolved 5 grains of copper, while 800 measures of nitrous gas were thrown into the retort, (probably the receiver.)

28. The following facts principally are established by the preceding experiments. 1. That a mixture of the vitriolic and nitrous acids in a concentrated state, has a peculiar faculty of dissolving silver copiously. 2. That it acts on, and principally calcines, tin, mercury, and nickel : the latter of which, however, it dissolves in small quantity : and that it has little or no action on other metals. 3. That the quantity of gas produced while the metal is dissolving is greater, relatively, to the quantity of metal dissolved, when the proportion of nitre to the vitriolic acid is small, than when large : and that when the metals are dissolved by mixtures, containing much nitre, and with a small production of gas, the solution itself, or the metallic salt formed in it, yields abundance of gas when mixed with water. 4. That dilution with water renders the concentrated mixture less capable of dissolving silver, but more capable of acting on other metals. 5. That this mixture of highly concentrated vitriolic and nitric acids, acquires a purple or violet colour when phlogisticated, either by addition of inflammable substances, as sulphur, or by its actions on metals, or by very strong impregnation of vitriolic acid with nitrous gas.* 6. That this phlogistication was found to communicate to the mixture the power of dissolving, though in small quantities, copper, iron, zinc, and the regulus of cobalt. 7. That water expels from a highly phlogisticated mixture of concentrated vitriolic and nitrous acids, or of oil of vitriol impregnated with nitrous gas, a great part of its contained gas ; and that therefore this gas is not capable of being retained in such quantity by dilute as by concentrated acids. Water unites with the mixture of vitriol and nitre, without any considerable effervescence.

29. To these observations I shall subjoin one other fact, namely, that when, to the mixture of oil of vitriol and nitre, a saturated solution of common salt in water is added, a powerful aqua regis is produced, capable of dissolving gold and platina ; and this aqua regis, though composed of liquors perfectly colourless and free from all metallic matter, acquires at once a bright and deep yellow colour. The addition of dry common salt to the concentrated mixtures of vitriolic and nitrous acids produces an effervescence but not the yellow colour ; for the production of which therefore a certain proportion of water seems to be necessary.

* Dr. Priestley has noticed this colour communicated to oil of vitriol by impregnation with nitrous gas or vapour, and also the effervescence produced by adding water to this impregnated liquor.

PART 2.—*On the precipitation of Silver from Nitrous Acid by Iron.*

§ 1. Bergman relates, that on adding iron to a solution of silver in nitrous acid, no precipitation ensued; though the affinity of iron to acids generally is known to be much stronger than that of silver; and though, even with regard to the nitrous acid, other experiments evince the superior affinity of iron; for as iron precipitates copper from this acid, and as copper precipitates silver, we must infer the greater affinity of iron than silver. In the course of his experiments, however, some instances of precipitation occurred, which he attributed to the peculiar quality of the irons which he employed.* I was desirous of discovering the circumstances, and of investigating the cause, of this irregularity and exception to the generally received laws of affinity.

2. I digested a piece of fine silver in pure and pale nitrous acid, and while the dissolution was going on, and before the saturation was completed, I poured a portion of the solution on a piece of clean and newly-scraped iron wire into a wine glass, and observed a sudden and copious precipitation of silver. The precipitate was at first black, then it assumed the appearance of silver, and was five or six times larger in diameter than the piece of iron wire which it enveloped. The action of the acid on the iron continued some little time, and then it ceased; the silver redissolved, and the liquor became clear, and the iron remained bright and undisturbed

* Bergman tried many different kinds of iron, and he thought he found two that were capable of precipitating silver. But as he did not discover the circumstances according to which the precipitation sometimes does, and at other times does not happen, he may have been mistakenⁿ with regard to the peculiar quality of these two kinds of iron. At least the several kinds which I have tried always precipitated silver in certain circumstances, and always failed to precipitate in certain other circumstances. I do not know any other author who has mentioned this subject, excepting Mr. Kirwan, who, in the conclusion of his valuable papers on the attractive powers on mineral acids, says "I have always found silver to be easily precipitated from its solution in the nitrous acid by iron. The sum of the quiescent affinities being 635, and that of the divellent 746. Yet Mr. Bergman observed, that a very saturated solution of silver was very difficultly precipitated, and only by some sorts of iron, even though the solution was diluted and an access of acid added to it. The reason of this curious phenomenon appears to me to be deducible from a circumstance first observed by Scheele in dissolving mercury, namely, that the nitrous acid when saturated with it will take up more of it in its metallic form. The same thing happens in dissolving silver in the nitrous acid in a strong heat; for, as I before remarked, the last portions of silver thrown in afford no air, and consequently are not dephlogisticated. Now this compound of calx of silver, and silver in its metallic form, may well be unprecipitable by iron, the silver in its metallic form preventing the calx from coming into contact with the iron, and extracting phlogiston from it." In this paper I shall not enter into the explanation of these appearances; but I thought it necessary to premise that what so eminent a chemist as Mr. Kirwan has suggested on the subject, that the reader may see at once the present state of the question. I shall only remark that the above explanation, not being founded on any peculiarity in the nature of iron, seems to suppose that the silver is also incapable of being precipitated from such solutions as iron, cannot act on by any other metal. But this is not the case; copper and zinc readily precipitate silver from these solutions.

in the solution at the bottom of the wine glass, where it continued during several weeks, without suffering any change, or effecting any precipitation of the silver.

3. When the solution of silver was completely saturated, it was no longer affected by iron, according to Bergman's observation.

4. Having found that the solution acted on the iron, and was thus precipitated, before it had been saturated, and not afterwards, I was desirous of knowing, whether the saturation was the circumstance which prevented the action and precipitation. For this purpose I added to a portion of the saturated solution some of the same nitrous acid, of which a part had been employed to dissolve the silver; and into this mixture, abounding with a superfluous acid, I threw a piece of iron, but no precipitation occurred. It was thence evident that the saturation of the acid was not the only circumstance which prevented the precipitation.

5. To another portion of the saturated solution of silver I added some red smoking nitrous acid; and I found, on trial, that iron precipitated the silver from this mixture, and that the same appearances were exhibited as had been observed with the solution before its saturation.

6. The same effects were produced when vitriolic acid was added to the saturated solution of silver, and iron afterwards applied.

7. To some of the same nitrous acid, of which a part had been employed to dissolve the silver, I added a piece of iron; and while the iron was dissolving I poured into the liquor some of the saturated solution of silver, on which a precipitation of silver took place instantly; though when the same acid had been previously mixed with the solution of silver, and the iron was then added to the mixture, no precipitation had ensued.

8. The quantity of vitriolic acid, or of the red fuming nitrous acid, necessary to communicate to the saturated solution of silver the property of being acted on by iron, varies according to the concentration, and to the degree of phlogistication of the acids added; so that a less quantity than is sufficient does not produce any apparent effect. Yet, when the solution is by the addition of these acids brought nearly to a precipitable state, the addition of spirit of wine will, in a little time, render it capable of acting on iron.

9. It appears then, that a solution of silver is not precipitated by iron in the cold, unless it have a superabundance of phlogisticated acid.*

* It was said, at section four, that the addition of dephlogisticated nitrous acid to a saturated solution of silver did not render this solution precipitable by iron. Yet, as this acid dissolves iron, such a quantity may be added, as to overcome the counteracting quality of the solution of silver, so that the acid shall be able to act on the iron; and while this metal is dissolving, it phlogisticates the mixture, which then becomes capable of being precipitated, and is in fact re-

10. Heat affects the action of a solution of silver on iron; for if iron be digested with heat, in a perfectly saturated solution of silver, such as a solution of crystals of nitre of silver in water, the silver will be deposited in its bright metallic state on different parts of the iron, and the iron which has been acted on by the solution appears in the form of a yellow ochre.

11. Bergman relates, that he has sometimes observed beautiful crystallizations or vegetations of metallic silver formed on pieces of iron immersed long in a solution of silver, I have found that no trial is able to effect this deposition, unless the solution be in a state nearly sufficiently phlogisticated to admit of a precipitation by iron, but not completely phlogisticated enough to effect that purpose immediately.

12. Dilution with a great deal of water seemed to dispose the solutions of silver to be precipitated by iron more easily. A solution of silver, which did not act on iron, on being very much diluted, and having a piece of iron immersed in it, during several hours, gave a precipitate of silver in the form of a black powder.

§ 2. *On the alterations which iron or its surface undergoes by the action of a solution of silver in nitrous acid, or of a pure concentrated nitrous acid.*—13. It has been said, that when iron is exposed to the action of a phlogisticated solution of silver, it instantly precipitates the silver, is itself acted on or dissolved by the acid solution during a certain time, longer or shorter, according to the degree of phlogistication, quantity of superabundant acid, and other circumstances, and that at length the solution of the iron ceases; the silver precipitate is redissolved, if there is superfluous acid; the liquor becomes clear again, but only rendered a little browner by having dissolved some iron; while the piece of iron remains bright and undisturbed at the bottom of the liquor, where it is no longer able to affect the solution of silver.

14. I poured a part of the phlogisticated solution of silver which had passed through these changes, and which had ceased to act on the piece of iron, into another glass, and dropped another piece of iron wire into the liquor; on which I observed a precipitation of silver, a solution of part of the iron, a redissolution of the precipitated silver, and a cessation of all these phenomena, with the iron remaining bright, and quiet at the bottom of the liquor, as before. It appeared then, that the liquor had not lost its power of acting

duced to the same circumstances as are described at section 7. The limits of the quantities which produce changes cannot be ascertained, because they depend on the degrees of concentration and phlogistication of the substances employed. and therefore, whenever a change is said to be produced by a certain substance, it means that it may be produced by some proportion, but does not imply by every proportion, of that substance. Without attending to these considerations, persons trying to repeat the experiments mentioned in this paper will be liable to be deceived.

on fresh iron, though it ceased to act on that piece which had been exposed to it.

15. To one of the pieces of iron which had been employed in the precipitation of a solution of silver, and from which the solution, no longer capable of acting on it, had been poured off, I added some phlogisticated solution of silver, which had never been exposed to the action of iron, but no precipitation happened. It appeared then, that the iron itself, by having been once employed to precipitate a solution of silver, was rendered incapable of any further action on any solution of silver. And it is to be observed, that this alteration was produced without the least diminution of its metallic splendour, or change of colour. The alteration however, was only superficial, as may be supposed; for by scraping off its altered coat, it was again rendered capable of acting on a solution of silver. To avoid circumlocution, I shall call iron thus affected, *altered iron*; and iron which is clean, and has not been altered, *fresh iron*.

16. To a phlogisticated solution of silver, in which a piece of bright *altered iron* lay, without action, I added a piece of *fresh iron*, which was instantly enveloped with a mass of precipitated silver, and acted on as usual; but, what is very remarkable, in about a quarter of a minute, or less, the *altered iron* was suddenly covered with another coat of precipitated silver, and was now acted on by the acid solution like the *fresh* piece. In a little time the silver precipitate was redissolved, as usual, and the two pieces of iron were reduced to an *altered* state. When a *fresh* piece was then held in the liquor, so as not to touch the two pieces of *altered* iron, they were also soon acted upon by the acid solution, and suddenly covered with silver precipitate as before; and these phenomena may be repeated with the same solution of silver, till the superfluous acid of the solution becomes saturated by the iron, and then the dissolution of the precipitated silver must cease.

17. I poured some dephlogisticated nitrous acid on a piece of *altered iron*, without any action ensuing, although this acid readily acted on *fresh iron*; and when, to the dephlogisticated nitrous acid, with a piece of *altered iron* lying immersed in it, I added a piece of *fresh iron*, this immediately began to dissolve, and soon afterwards the *altered iron* was acted on also by the acid.

18. On a piece of *altered iron* I poured a solution of copper in nitrous acid; but the copper was not precipitated by the iron; neither did this iron precipitate copper from a solution of blue vitriol.

19. *Altered iron* was acted on by a dilute phlogisticated nitrous acid; but not by a red concentrated acid, which is known to be highly phlogisticated.

20. I put some pieces of clean fresh iron wire into a concentrated

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and red fuming nitrous acid. No apparent action ensued, but the iron was found to be altered in the same manner as it is by a solution of silver: that is, it was rendered incapable of being attacked either by a phlogisticated solution of silver, or by dephlogisticated nitrous acid.

21. Iron was also *altered* by being immersed some little time in a saturated solution of silver, which did not show any visible action on it.

22. The alteration thus produced on the iron is very superficial. The least rubbing exposes some of the *fresh iron* beneath its surface, and thus subjects it to the action of the acid. It is therefore with difficulty that these pieces of *altered iron* can be dried without losing their peculiar property. For this reason, I generally transferred them out of the solution of silver, or concentrated nitrous acid, into any other liquor, the effects of which I wanted to examine. Or they may be transferred first into a glass of water, and then into the liquor to be examined. But it is to be observed, that if they be allowed to remain long in the water, they lose their peculiar property or alteration. They may be preserved in their altered state by being kept in spirit of sal ammoniac.

23. To a saturated solution of copper in nitrous acid, which was capable of being readily precipitated by fresh iron, I added some saturated solution of silver. From this mixture a piece of *fresh iron* neither precipitated silver nor copper: nor did the addition of some dephlogisticated nitrous acid effect this precipitation.

24. A solution of copper, formed by precipitating silver from nitrous acid by means of copper, was very reluctantly and slowly precipitated by a piece of *fresh iron*; and the iron thus acted on by the acid was changed into an ochre.

25. A saturated solution of silver having been partly precipitated by copper, acquired the property of acting on *fresh iron*, and of being preprecipitated by it.

26. Fresh iron immersed sometime in solutions of nitre or lead, or of nitre of mercury in water, did not occasion any precipitation of the dissolved metals; but acquired an altered quality. These metals then in this respect resemble silver.

27. It is well known, that a solution of martial vitriol, added to a solution of gold in aqua regis, precipitates the gold in its metallic state. I do not recollect, that the precipitation of a solution of silver, by the same vitriol, has been observed. However, on pouring a solution of martial vitriol into a solution of silver in the nitrous acid, a precipitate will be thrown down, which acquires, in a few minutes, more and more of a metallic appearance, and is indeed perfect silver. When the two solutions are partly concentrated, a bright argentine film swims on the surface of the mixture, or silvers the side of the glass in which the experiment is made. When a

phlogisticated solution of silver is used, the mixture is blackened, as happens generally to a solution of martial vitriol, where phlogisticated nitrous acid is added to it.

I added about equal parts of water to a mixture of a phlogisticated solution of silver and a solution of martial vitriol, in which all the silver had been precipitated, and digested the diluted mixture with heat, by which means most of the precipitated silver was redissolved. Bergman has observed a similar redissolution of gold precipitated by martial vitriol on boiling the mixture: but he attributes the redissolution to the concentration of the aqua regis by the evaporation. As this explanation did not accord with my notions, I diluted the mixture with water, and found that the same redissolution occurred both with the solution of silver and with that of gold. But with neither of the metals did I find that the redissolution took place, unless there had been a superabundant acid in the solutions of silver and gold employed.

28. Mercury is also precipitated in its metallic state from its solution in nitrous acid, by a solution of martial vitriol. When the liquor is poured off from its precipitate, this may be changed into running mercury by being dried near the fire.

29. I found also that silver may be precipitated in its metallic state, from its solution in vitriolic acid, by addition of a solution of martial vitriol. A vitriol of mercury may also be decomposed by a solution of martial vitriol, and the mercurial precipitate, which is a black powder, forms globules, when dried and warmed.

30. Luna cornea is not decomposed by martial vitriol; consequently there is no operation of a double affinity. Yet this luna cornea may be decomposed by the elements of martial vitriol, while they are in the act of dissolution: that is, the silver may be precipitated in its metallic state, by digesting luna cornea with a dilute vitriolic acid, to which some pieces of iron are added. And it is to be observed, that this reduction of the silver and precipitation take place, while the acid is yet unsaturated. Marine acid and iron applied to luna cornea effect the same reduction of the silver to a metallic state, even when there is more acid than is sufficient for both metals.

The explanation of these phenomena will be attempted in the subsequent papers which I propose to present on this subject to the society. *

* We are not aware that Mr. Keir ever favoured the scientific world with the explanation here proposed.—EDIT.

LIV.—*Brief Synopsis of the Principles of MR. JAMES P. ESPY'S
Philosophy of Storms.**

When the air near the surface of the earth becomes more heated or more highly charged with aqueous vapour, which is only five-eighths of the specific gravity of atmospheric air, its equilibrium is unstable, and up-moving columns or streams will be formed.

As these columns rise, their upper parts will come under less pressure, and the air will therefore expand; as it expands, it will grow colder about one degree and a quarter for every hundred yards of its ascent, as is demonstrated by experiments on the Nephelescope.

The ascending columns will carry up with them the aqueous vapour which they contain, and if they rise high enough, the cold produced by expansion from diminished pressure, will condense some of this vapour into cloud; for it is known that cloud is formed in the receiver of an air-pump when the air is suddenly withdrawn.

The distance or height to which the air will have to ascend before it will become cold enough to begin to form cloud, is a variable quantity depending on the number of degrees which the dew point is below the temperature of the air; and this height may be known at any time by observing how many degrees a thin metallic tumbler of water must be cooled down below the temperature of the air, before the vapour begins to condense on the outside. The highest temperature at which it will condense, which is variable according as there is more or less vapour in the air, is called the "dew point," and the difference between the dew point and the temperature of the air in degrees is called the complement of the dew point.

It is manifest that if the air at the surface of the earth should at any time be cooled down a little below the dew point, it would form a fog by condensing a small portion of its transparent vapour into little fine particles of water, and if it should be cooled 20° below the dew point, it would condense about one half its vapour into water, and at 40° below, it would condense about three-fourths of its vapour into water, &c.

This, however, will not be exactly the case from the cold produced by expansion in the up-moving columns; for the vapour itself grows thinner, and the dew point falls about one-quarter of a degree for every hundred yards of ascent.

It follows, then, as the temperature of the air sinks about one degree and a quarter for every hundred yards of ascent, and the

* Copious facts going to establish the principles contained in this Synopsis are given in Mr. Espy's Lectures.

dew point sinks about a quarter of a degree, that as soon as the column rises as many hundred yards as the complement of the dew point contains degrees of Fahr., cloud will begin to form. Or in other words, the bases of all clouds forming by the cold of diminished pressure from up-moving columns of air, will be about as many hundred yards high as the dew point is below the temperature of the air at the time.

If the temperature of the ascending column should be 10° above that of the air through which it passes, and should rise to the height of 4800 feet before it begins to form cloud, the whole column would then be 100 feet of air lighter than surrounding columns; and if the column should be very narrow, its velocity of upward motion would follow the laws of spouting fluids, which would be eight times the square root of 100 feet a second, that is 80 feet a second, and the barometer in the centre of the column at its base, would fall about the ninth of an inch.

As soon as cloud begins to form, the caloric of elasticity of the vapour or steam is given out into the air in contact with the little particles of water formed by the condensation of the vapour. This will prevent the air in its further progress upwards from cooling so fast as it did up to that point, and from experiments on the Nepheloscope, it is found to cool only about one half as much above the base of the cloud as below—that is, about five-eighths of a degree for one hundred yards of ascent, when the dew point is about 70° . If the dew point is higher it cools a little less, and if the dew point is lower, it cools a little more than five-eighths of a degree in ascending one hundred yards.

Now it has been ascertained by aéronauts and travellers on mountains, that the atmosphere itself is about one degree colder for every hundred yards in height above the surface of the sea; therefore, as the air in the cloud, above its base, is only five-eighths of a degree colder for every hundred yards in height, it follows, that when the cloud is of great perpendicular height above its base, its top must be much warmer than the atmosphere at that height, and consequently much lighter.

Indeed the specific gravity of a cloud of any height compared to that of the surrounding air at the same elevation, may be calculated when the dew point is given. For its temperature is known by experiments with the Nepheloscope, and the quantity of vapour condensed by the cold of diminished pressure at every point in its upward motion, and of course the quantity of caloric of elasticity given out by this condensation is known, and also the effect this caloric has in expanding the air receiving it, beyond the volume it would have, if no caloric of elasticity was evolved in the condensation of the vapour.

For example, according to the experiments of Prof. Walter R. Johnson, of Philadelphia, a pound of steam at the temperature of

212° contains 1030 of caloric of elasticity, and as the sum of the latent and sensible caloric of steam is the same at all temperatures, it follows that a pound of steam being condensed into 1198 pounds of water at 32° would heat it up 1°. And as the specific caloric of air is only 0.267, if a pound of vapour should be condensed into 1198 pounds of air, it would heat that air nearly 4°, or which is the same thing, it would heat 119 pounds of air 4°, or 100 pounds 48°, and in all these cases it would expand the air about 8000 times the bulk of water generated; that is, 8000 cubic feet for every cubic foot of water formed out of the condensed vapour. And as it requires between 1300 and 1400 cubic feet of vapour, at the ordinary temperature of the atmosphere, to make one cubic foot of water—if this quantity be subtracted from 8000, it will leave upwards of 6600 cubic feet of actual expansion of the air in the cloud for every cubic foot of water generated there by condensed vapour.

This great expansion of the air in the forming cloud will cause the air to spread outwards in all directions above, causing the barometer to rise on the outside of the cloud above the mean, and to fall below the mean under the middle of the cloud as much as it is known to do in the midst of great storms.

For example, if the dew point should be very high, say 78°, then the quantity of vapour in the air would be about one-fiftieth of its whole weight, and if the up-moving column should rise high enough to condense one-half its vapour into cloud, it would heat the air containing it 45°, and the air so heated would be 45-480ths larger than it would be if it was not so heated. And if we assume a case within the bounds of nature, and suppose the cloud and the column under the cloud to occupy three-fourths of the whole weight of the atmosphere, or in other words, if we suppose the top of the cloud to reach a height where the barometer would stand at 7½ inches, and the mean temperature of the whole column 40° warmer than the surrounding air, then would the barometer fall under the cloud at the surface of the earth 40-480ths of 22.5, or a little more than two inches.

Though the air may be driven up by the ascending column much higher than the point assumed in the last article, the cloud will cease to form at greater heights, because the dew point at these great elevations, falls by a further ascent as rapidly as the temperature—and at greater elevations, it will even fall more rapidly. If for instance the air should rise from where the barometer stands at six inches to where it stands at three inches, the dew point would fall about 20°, but the temperature would fall less than 20°, and therefore no vapour would be condensed by such ascent.

When a cloud begins to form from an ascending column of air, it will be seen to swell out at the top while its base continues on the same level, for the air has to rise to the same height before it

becomes cold enough by diminished pressure to begin to condense its vapour into water; this will cause the base to be flat, even after the cloud has acquired great perpendicular height, and assumed the form of a sugar loaf. Other clouds also for many miles around, formed by other ascending columns, will assume similar appearances, and will moreover have their bases all on the same or nearly the same horizontal level; and the height of these bases from the surface of the earth will be the greatest about three o'clock, when the dew point and temperature of the air is the greatest distance apart.

The outspreading of the air in the upper parts of an ascending column will form an annulus all round the cloud, under which the barometer will stand above the mean; of course the air will descend in the annulus, and increase the velocity of the wind at the surface of the earth towards the centre of the ascending column, while all round on the outside of the annulus there will be a gentle wind outwards. Any general currents of air which may exist at the time, will of course modify these motions from the oblique forces they would occasion.

The up-moving current of air must of course be entirely supplied by the air within the annulus, and that which descends in the annulus itself.

The rapid disturbance of equilibrium, which is produced by *one* ascending column, will tend to form *others* in its neighbourhood; for the air being pressed outwards from the annulus, or at least retarded on the windward side, will form other ascending columns, and these will form other annuli, and so the process will be continued.

These ascending columns will have a tendency to approach, and finally unite; for the air between them must descend, and in descending the temperature of the whole column will increase, for it is known that the air, at great elevation, contains more caloric to the pound than the air near the surface of the earth, because it is the upper regions that receive the caloric of elasticity given out in the condensation of vapour into clouds. Therefore, when the air has descended some time in the middle, between two ascending columns, the barometer will fall a little, or at least not stand so high above the mean as it does on the outside of the two clouds, and so the columns will be pressed towards each other.

If one of two neighbouring columns should be greatly higher than the other, its annulus may overlap the smaller one, and of course the current under the smaller cloud will be inverted, and the cloud which may have been formed over the column thus forced to descend, will soon disappear; for as it is forced downwards by the overlapping annulus of the more lofty column, it will come under greater pressure, and its temperature will be thus increased, and it is manifest that as soon as its top descends as low as

its base, it will have entirely disappeared, and in the mean time the larger cloud will have greatly increased.

As the air above the cloud formed by an ascending column is forced upwards, if it contains much aqueous vapour, a thin film of cloud will be formed in it by the cold of diminished pressure, entirely distinct from the great dense cumulus below; but as the cumulus rises faster than the air above it (for some of the air will roll off) the thin film and the top of the cumulus will come in contact; and sometimes a second film or cap may be formed in the same way, and perhaps a third and fourth. When these caps form, there will probably be rain, as their formation indicates a high degree of saturation in the upper air.

When the complement of the dew point is very great (20° and more,) clouds can scarcely form; for up-moving columns will generally either come to an equilibrium with surrounding air, or be dispersed before they rise twenty hundred yards, which they must do in this case, before they form clouds. Sometimes, however, masses of air will rise high enough to form clouds, but they are generally detached from any up-moving column underneath, and of course cannot then form cumuli with flat bases; such clouds will be seen to dissolve as soon as they form, and even while forming they will generally appear ragged, thin and irregular.

Moreover, if the ground should be colder during the day, than the air in contact with it, as it sometimes happens after a very cold spell of weather, then ascending columns cannot exist, and of course no cumuli can be formed on that day, even though the air may be saturated with vapour to such a degree as to condense a portion of it on cold bodies at the surface of the earth.

Neither can clouds form of any very great size, when there are cross currents of air sufficiently strong to break in two an ascending current, for the ascensional power of the up-moving current will thus be weakened and destroyed. This is one means contrived by nature to prevent up-moving columns from increasing until rain would follow. Without some such contrivance it is probable that every up-moving column which should begin to form cloud when the dew point is favourable, would produce rain, for as soon as cloud forms, the up-moving power is rapidly increased by the evolution of the caloric of elasticity.

If it should be found by observation that an upper current of air is passing from the mountains of Abyssinia over Egypt to the north, while the wind below is blowing from the north towards the mountains of Abyssinia, this would manifestly be *one* reason why it seldom rains in Egypt during the prevalence of this wind, though it comes highly charged with vapour from the Mediterranean. Besides, it is known that during the prevalence of this wind there are great rains in Abyssinia, and of course if the upper current does flow over Egypt from the south, it would bring in it a large portion

of the caloric of elasticity, which it received there, in the great condensation of the vapour as it rose up the sides of the mountains at the head of the Nile; of course the columns of air rising over Egypt, when they entered that current would cease to rise, for the temperature of that current would be many degrees hotter than themselves, and therefore they could not swim in it.

Also, on the leeward side of very lofty mountains, there cannot be rain: for as the air on the windward side rises up the sides of the mountain, it will condense all the vapour which can be condensed by the cold of diminished pressure, before it reaches to the top, and even if a cloud passes over the top to the other side, it would soon disappear, because in passing down the slope it will come under greater pressure, and thus be dissolved by the heat produced. These are some of the causes which prevent rains at particular times and in particular localities.

If, however, the air is very hot below with a high dew point, and no cross currents of air above to a great height, then, when an up-moving current is once formed, it will go on and increase in violence, as it acquires perpendicular elevation, especially after the cloud begins to form. At first the base of the cloud will be flat; but after the cloud becomes of great perpendicular diameter, and the barometer begins to fall considerably, as it will do from the specific levity of the air in the cloud, then the air will not have to rise so far as it did at the moment when the cloud began to form, before it reaches high enough to form cloud from the cold of diminished pressure.

The cloud will now be convex below, and its upper parts will be seen spreading outward in all directions, especially on that side towards which the upper current is moving, assuming something of the shape of a mushroom. In the mean time the action of the inmoving current below and upmoving current in the middle will become very violent, and if the barometer falls two inches under the centre of the cloud, the air will cool about 10° , and the base of the cloud will reach the earth if the dew point was only 8° below the temperature of the air at the time the cloud began to form. The shape of the lower part of the cloud will now be that of an inverted cone with its apex on the ground, and it will be what is called a tornado if it is on land, and a water-spout if at sea.

On visiting the path of a tornado, the trees on the extreme borders will all be found prostrated with their tops inwards, either inwards and backwards, or inwards and forwards, or exactly transverse to the path. The trees in the centre of the path will be thrown either backwards or forwards parallel to the path; and invariably if one tree lies across another, the one which is thrown backwards is underneath. Those materials on the sides which are moved from their places and rolled along the ground, leaving a trace of their motion, will move in a curve convex behind; those

which were on the right hand of the path, will make a curve from left hand to right, and those on the left hand of the path will make a curve from right hand to left, and many of these materials will be found on the opposite side of the path from that on which they stood on the approach of the tornado. Also, those bodies which are carried up will appear to whirl, unless they arise from the very centre—those that are taken up on the right of the centre will whirl in a spiral from left to right, and those on the left of the centre, will whirl in a spiral upwards from right to left. On examining the trees which stand near the borders of the path, it will be found that many of the limbs are twisted round the trees and broken in such a manner as to remain twisted, those on the right hand side of the path from left to right, and those on the left hand side of the path from right to left. However, it will be found that only those limbs which grew on the side of the tree most distant from the path of the tornado are broken; for these alone were subject to a transverse strain.

The houses which stood near the middle of the path will be very liable to have the roof blown up, and many of the walls will be prostrated all outwards, by the explosive influence of the air within, and those houses covered with zinc or tin, from being air tight will be liable to suffer most. The floors from the cellars will also frequently be thrown up, and the corks of empty bottles exploded.

All round the tornado at a short distance, probably not more than three or four hundred yards, there will be a dead calm, on account of the annulus formed by the rapid efflux of air above, from the centre of the up-moving and expanding column. In this annulus the air will be depressed, and all round on the outside of it, at the surface of the earth, there will be a gentle wind outwards; and of course all the air which feeds the tornado, is supplied from within the annulus. Nor is this difficult to understand, when the depression of the air in the annulus is considered, for any amount may be thus supplied by a great depression.

Light bodies, such as shingles, branches of trees, and drops of rain or water formed in the cloud, which are carried up to a great height before they are permitted to fall to the earth; for though they may frequently be thrown outwards above, and then descend a considerable distance at the side, they will meet with an inblowing current below, which will force them back to the centre of the up-moving current, and so they will be carried aloft again.

The drops of rain, however, will frequently be carried high enough to freeze them, especially if they are thrown out above so far as to fall into clear air, for this air will in some cases be thirty or forty degrees colder than the air in the cloud. In this case if the up-moving column is perpendicular, the hail will be thrown out on both sides, and on examination it will be found that two veins of hail fell simultaneously, at no great distance apart.

It is indeed probable that in all violent thunder storms in which hail falls, the upmoving current is so violent as to carry drops of rain to a great height, when they freeze and become hail. It is difficult if not impossible to conceive any other way in which hail can be formed in the summer, or in the torrid zone.

In those countries in which an upper current of air prevails in a particular direction, the tornadoes and water-spouts will generally move in the same direction; because the upmoving column of air in this meteor rises far into this upper current, and of course its upper part will be passed in this direction, as the great tornado cloud moves on in the direction of the upper current, the air at the surface of the earth will be pressed up into it by the superior weight of the surrounding air. It is for this reason the tornado in Pennsylvania generally moves towards the eastward.

If a tornado should stop its motion for a few seconds, as it might do, on meeting with a mountain, it would be likely to pour down an immense flood of water or ice, in a very small space; for the drops which would be carried up by the ascending current would soon accumulate to such a degree, as to force their way back, and this they could not do, without collecting into one united stream of immense length and weight, and of course on reaching the side of the mountain, this stream, whether it consisted of water or hail, would cut down into the side of the mountain a deep hole, and make a gully all the way to the bottom of the mountain from the place where it first struck.

As the air spreads out more rapidly above than it runs in below, there will be a tendency in storms to increase in diameter, and also to become oblong from the influence of the upper current in carrying the top of the cloud in its own direction.

At the equator, or at least those parts of it where the trade winds are constant from east to west, it is probable tornadoes travel from east to west. For as the air in the torrid zone is about 80° in temperature at a mean, and the air in the frigid zone is about zero, the air in the torrid zone is constantly expanded by heat about 80-448th of its whole bulk in the frigid zone. This will cause the air at the equator to stand more than seven miles higher from the surface of the earth to the top of the atmosphere than at the north pole. The air therefore will roll off from the torrid zone both ways towards the poles, causing the barometer to fall in low latitudes and rise above the mean in high latitudes. This will cause the air to run in below towards the equator, and of course rise there. Now from the principle of the conservation of areas, it will fall more and more to the west as it rises, and of course the upper current of the air, at the equator, probably moves towards the west.

However, as the air rolls off above, towards the north, it will be constantly passing over portions of the earth's surface, which have a less diurnal velocity than the part from which it set out, and as

from the nature of inertia it still inclines to retain the diurnal velocity towards the east which it originally possessed, when it reaches the latitude of about 20° or 25° , it will then probably be moving nearly towards the north—and beyond that latitude its motion will be to the northeasterly.

If violent storm clouds, which necessarily rise to a great height into the upper current, are driven forward in the direction of the upper current, it is probable that the barometer will rise higher in that part of the annulus which is in front of the storm, than in the rear, and if so, a sudden rise of the barometer in particular localities, may become, when properly understood, one of the first symptoms of an approaching storm.

In consequence of the high barometer in front of the storm in a semi-annulus, the air will be forced downwards there, and cause in some cases a more violent action of the air or wind backwards, meeting the approaching storm, than will be experienced, in the rear of the storm.

As the air comes downwards in the semi-annulus in front of the storm, it will come under greater pressure, and therefore any clouds which it may contain, will probably be dissolved, by the heat of great pressure, and therefore on the passage of the annulus, it will probably be fair weather.

Also, as the air above always contains more caloric to the pound, than the air below, there will be an increase of temperature on the passage of the annulus, partly from the increased pressure, but chiefly by the descent of the air; in very hot climates this increase of temperature, in front of the storm, will be very sensibly felt.

The increased pressure in the annulus round a volcano, when it suddenly bursts out, will sometimes under favourable circumstances, be very great, and of course the air will be depressed from a great height, so that some portion of the very air which has gone up in the central parts of the ascending column, and formed cloud by the cold of diminished pressure, will be forced down to the surface of the earth, bringing with it the caloric of elasticity which it received from the condensing vapour; if so, the heat experienced at the time of this descent, will be very great.

These hot blasts of air will alternate with cold blasts, for the air which is forced down from great heights in the annulus will not only be very hot, but very dry, having condensed its vapour, in its previous ascent. Now when this hot dry air flows inwards again towards the volcano and ascends, it will not form cloud, because of its want of vapour; and therefore the process of cloud forming will cease, and consequently rain and hail will cease too, until more air from a greater distance that has not been deprived of its vapour flows in and ascends. Then cloud will again begin to form and the violence and rapidity of the outflowing of the air above will be increased by the evolution of the caloric of elasticity.

the barometer will rise rapidly in the annulus and fall in the central part of the ascending column, and these alternations may continue while the volcano is in activity, more particularly if the violence of the volcano itself should be increased periodically.

As air cannot move upwards without coming under diminished pressure, and as it must thus expand and grow colder and consequently form cloud—any cause which produces an upmoving column of air, whether that cause be natural or artificial, will produce rain, when the complement of the dew point is small, and the air calm below and above, and the upper part of the atmosphere of its ordinary temperature.

Volcanoes therefore under favorable circumstances will produce rain; sea breezes which blow inwards every day towards the centre of islands, especially if these islands have in them high mountains, which will prevent any upper current of air from bending the upmoving current of air out of the perpendicular before it rises high enough to form cloud, such as Jamaica—will produce rain every day; great cities where very much fuel is burnt, in countries where the complement of the dew point is small, such as Manchester and Liverpool, will frequently produce rain; even battles, and accidental fires, if they occur under favorable circumstances, may sometimes be followed by rain. Let all these favorable circumstances be watched for in time of drought, (and they can only occur then,) and let the experiment be tried. If it should be successful, the result would be highly beneficial to mankind.

Independent of its utility to the farmer, it would be highly useful to the mariner in the following way.

As the very time and place of the commencement of the rain would be known, it would be easy to find out in what direction from the place of beginning it moved along the surface of the earth, and also its velocity of motion, and the shape that it assumed, from time to time in its progress. Now this knowledge is the principal thing wanting to enable the mariner, who has the power of locomotion, to direct his vessel so, when one of these great storms comes near him, as to use as much wind in the borders of the storm as will suit the purposes of navigation—for heaven undoubted makes the wind blow for his use and not for his destruction, provided he becomes acquainted with the laws to which it is subject. From the preceding principles he will be able to know in what direction a great storm is raging when it is yet several hundred miles from him.—*From Silliman's Journal.*

LV.—*On the Electricity of a Jet of Steam issuing from a Boiler.*

By H. G. ARMSTRONG, Esq., in *Letters to Professor Faraday*.*

SIR,

A few days ago, I was informed that a very extraordinary electrical phenomenon, connected with the efflux of steam from the safety-valve of a steam-engine boiler, had been observed at Seghill, about six miles from Newcastle. I therefore took an early opportunity of going over to that place, to investigate the truth of what I had heard, and by so doing I have ascertained the precise facts of the case, which appear to me to possess so much novelty and importance, that I deem it right to transmit the particulars to you believing that in your hands they will prove most conducive to the advancement of science. Without further preface, I shall proceed to narrate what I saw and heard on the spot.

There is nothing remarkable in the construction of the boiler, which is supported upon brick-masonry in the usual way. The safety-valve is placed on the top of a small cylinder, having a flange round the lower end, which is fastened by bolts to the summit of the boiler, between which and the flange, a cement, composed of chalk, oil, and tow, is interposed for the purpose of making the joining steam-tight.

About three weeks ago the steam began to escape at this joining, through a fissure in the cement, and has ever since continued to issue from the aperture in a copious horizontal jet. Soon after this took place, the engine-man, having one of his hands accidentally immersed in the issuing steam, presented the other to the lever of the valve, with a view of adjusting the weight, when he was greatly surprised by the appearance of a brilliant spark, which passed between the lever and his hand, and was accompanied by a violent wrench in his arms, wholly unlike what he had ever experienced before. The same effect was repeated when he attempted to touch any part of the boiler, or any iron-work connected with it, provided his other hand was exposed to the steam. He next found that while he held one hand in the jet of steam, he communicated a shock to every person whom he touched with the other, whether such person were in contact with the boiler, or merely standing on the brickwork which supports it; but that a person touching the boiler, received a much stronger shock than one who merely stood on the bricks.

These singular effects were witnessed and experienced by a great many persons, and among others by two gentlemen with whom I am personally acquainted, and who fully corroborate the above account, which I obtained from the engine-man.

The boiler had been cleaned out the day before I saw it, and a

* From the *Philosophical Magazine*.

thin incrustation of calcareous matter reaching as high as the water level had been removed, and the consequence was, that the indications of electricity, though still existing, were very much diminished. Still, however, what remained was very extraordinary; for when I placed one hand in the jet of steam and advanced the other within a small distance of the boiler, a distinct spark appeared, and was attended with a slight electrical shock.

From the effect produced by the cleaning of the boiler, it appears pretty obvious that the phenomenon is in a great measure, though not wholly, dependent upon the existence of an incrustation within; and the reason why such effects do not in any degree attend the effluxion of a jet of steam from a boiler in ordinary cases, must, I apprehend, be sought for in the fact, that in the present instance the steam escapes through an aperture in a non-conducting material, while in a vast majority of cases the escape must take place through a metallic orifice. Can the explosion of boilers, respecting the cause of which so much uncertainty at present exists, have any connexion with the rapid production of electricity which thus appears to accompany the generation of steam?

In the present case the incrustation of the boiler is very rapidly formed, and I therefore expect that in a few days the effects will have become as strong as they were at first. Whenever this takes place I shall again go over to witness them, and if you wish for any further information, I shall be glad to obtain it for you. In the mean time you are at liberty to make any use of this letter that you think fit.

I am, Sir, very respectfully yours,

H. G. ARMSTRONG.

Newcastle-upon-Tyne, Oct. 14, 1840.

Newcastle-upon-Tyne, Oct. 22, 1840.

Dear Sir,—I yesterday revisited the boiler at Seghill, in company with some friends, and took with me such apparatus as I deemed necessary for experimenting on the electrical steam. The results of this second visit I now hasten to communicate, and you will find in the following account of my proceedings, answers to all the queries you were kind enough to send me, for the purpose of directing my attention to the proper points of inquiry.

I found the boiler, and every thing connected with it, precisely in the state in which I have already described it, and on trying the steam in the same way as I did on the former occasion, the effect was very nearly the same; but when I placed myself on an insulating stool, the intensity of the sparks which passed between my hand and the boiler was greatly increased, as well as the twitching sensation in the knuckles and wrist, which accompanied the opera-

tion, and which in my former letter I designated a slight electrical shock. In pursuance of your instructions, I had provided myself with a brass plate, having a copper wire attached to it, which terminated in a round brass knob. When this plate was held in the steam by means of an insulated handle, and the brass knob brought within about a quarter of an inch from the boiler, the number of sparks which passed in a minute was from sixty to seventy, as nearly as we could count; and when the knob was advanced about one-sixteenth of an inch nearer to the boiler, the stream of electricity became quite continuous. The greatest distance between the knob and the boiler, at which a spark would pass from one to the other, was fully an inch. A Florence flask, coated with brass filings on both surfaces, was charged to such a degree with the sparks from the knob, as to cause a spontaneous discharge through the glass; and several robust men received a severe shock from a small Leyden jar charged by the same process. The strength of the sparks was quite as great when the knob was presented to any conductor communicating with the ground, as when it was held to the boiler. It appeared to make very little difference in what part of the jet the plate attached to the conducting wire was held; but when a thick iron wire was substituted for the plate, the effect was greatest when the wire was held very near to the orifice. The valve was loaded at the rate of 35lbs per square inch; but the pressure of the steam fluctuated considerably, which gave me an opportunity of observing that the quantity of electricity derived from the jet increased and diminished with the pressure. The electricity of the steam was *positive*, for when the pith balls of the electrometer diverged upon an instrument connected with the steam, they were attracted by a piece of sealing wax rubbed on woollen cloth; and when a pointed wire was held by the person on the stool, under the shade of a hat, a *pencil*, and not a *star*, of electrical light became visible.

Besides the principal jet of steam which I operated upon, there were several small streams issuing from different parts of the boiler, and in each of these the electrometer indicated the presence of electricity. From the peculiar manner in which the steam blew off from the safety-valve when the weight on the lever was lifted, it was quite impossible to try any satisfactory experiment upon the steam which was allowed to escape by that means. I applied the gold leaf electrometer to various parts of the boiler, which, I ought to observe, is in direct communication with the ground by means of the steam pipes, but could scarcely detect a trace of electricity in any part of it.

The engine has another boiler besides the one in question, and the two boilers lie immediately adjacent to each other. Having been informed that similar phenomena had been discovered in this second boiler, I proceeded to apply the electrometer to some small pencils of steam which were escaping in different parts, and found

the same indications which I had observed under similar circumstances in the first boiler. I then raised the safety-valve, and the column of steam which escaped from it proved as highly charged with electricity as the horizontal jet which issued from the other boiler, and in which the phenomenon had first been observed.

Upon inquiry, I found that the water used in the boilers was obtained from a neighbouring colliery, where it was pumped out of the mine, and that the same water was used for the boiler of a small high-pressure engine adjoining the colliery from which the water was procured. In order, therefore, to form an opinion whether or not the phenomena in question was dependent upon the quality of the water from which the steam was generated, I proceeded to examine the steam evolved from the boiler to which I had been referred, and which proved to be a very small one. The valve was loaded with only twenty pounds on the square inch, and I learned from the engine-man that no appearance of electricity had ever been noticed in the steam. Upon trial, however, I succeeded in obtaining very distinct sparks of electricity from the column of steam which issued from the safety-valve. The sparks were certainly weaker than those obtained at the other engine, but this may reasonably be ascribed to the inferior pressure of the steam, and smaller size of the boiler.

I then repaired to another high-pressure engine, which belonged to the same establishment, and the boiler of which was supplied with *rain* water instead of that drawn from the mine. In this case the pressure of the steam was forty pounds on the square inch. The valve was inaccessible, but a powerful jet of steam was obtained from the upper guage-cock; I could not, however, obtain any trace of electricity in the steam from this boiler, not even sufficient sensibly to affect the gold-leaf electrometer. The presumption, then, is exceedingly strong, that the phenomenon is in some way occasioned by the peculiar nature of the water from which the steam is produced. I enclose you a specimen of the incrustation*, of a month's growth, deposited by the water from the mine in the boilers in which it is used.

I shall be glad to receive any further instructions from you as to the proper mode of pursuing the investigation, and should be much gratified to hear your opinion as to the cause of this most curious phenomenon †.

I am, dear sir,

Very respectfully yours,

H. G. ARMSTRONG.

M Faraday, Esq.

* The incrustation is grey and hard; it contains traces of a soluble muriate and sulphate, but consists almost entirely of sulphate of lime, with a little oxide of iron and insoluble clayey matter, carried in probably by the water. There is hardly a trace of carbonate of lime in it.—M. F.

† The evolution of electricity by vaporization, described by Mr. Armstrong

LVI.—*Experiments on the Electricity of High-Pressure Steam.* By
H. L. PATTINSON, Esq., F.G.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

A very singular phenomenon, viz., the production of electricity by two steam-boilers, has been observed in this neighbourhood within the last few weeks, the particulars of which I have the pleasure of transmitting to you for publication in your valuable Journal. The boilers in question are situated at Cramlington Colliery, eight miles north-east of Newcastle, where they supply steam to a high-pressure engine of 28-horse power, employed on the waggon-way to haul full and empty waggons to the top of two inclined planes, leading to the colliery on the one hand, and to the river Tyne on the other. The boilers are cylindrical, with circular ends, each twenty-one feet long, and five feet diameter. They are supplied with water from an adjacent pond by iron feed-pipes, four inches diameter, and the steam they produce is conveyed to the working cylinder by other iron pipes, six inches diameter, which pipes form also a direct metallic communication between the tops of the boilers. By means of appropriate valves the steam is supplied to the cylinder from one or other boiler at pleasure. A pipe, two inches diameter, leads from the bottom of one boiler on the outside of the brick-work to the ash-pit, through which the sediment deposited by the water is occasionally blown from one of Scott's patent collecting cones, and a similar pipe is attached to the other boiler. The boilers are set in brick-work in the usual way, the fires below, with flues reaching all round, and passing into the chimney also in the usual manner. The flues are covered with large flat bricks, and in the space between the boilers the two flues are necessarily separated by a brick wall. The safety-valves are attached to the boilers by flange joints; and between the flanges, to render them steam-tight, is placed a ring of plaited hemp covered with a cement of litharge, sand and linseed oil, mixed up together, and when applied of the consistence of glaziers' putty. This cement, as it soon becomes hard, is used about the engine for steam joints which occasionally fail; but all the joints of the pipes are made of iron borings and sal-ammoniac, as ordinarily employed by

is most likely the same as that already known to philosophers on a much smaller scale, and about which there are as yet doubts whether it is to be referred to a mere evaporation, as Harris says, or to chemical action, according to others. This point it neither settles nor illustrates; but it gives us the evolution of electricity during the conversion of water into vapour, upon an enormous scale, and therefore brings us much nearer to the electric phenomena of volcanos, water-spouts and thunder-storms, than before.—M. F.

engine-wrights. The steam is worked at a pressure of thirty-five pounds per inch.

The joint between the top of one of the boilers and the seat of its safety-valve had given way, and steam was issuing forcibly through this aperture, when on Tuesday, September 29th last, the engine-man, William Patterson, while standing with this current of steam blowing upon his legs, took hold of the weight attached to the lever of the safety-valve, to try the strength of the steam, when he felt a peculiar pricking sensation in the ends of his fingers, but as the steam prevented him from seeing distinctly, he thought he had merely struck his fingers rather suddenly against the weight. On Friday, October 2nd, on taking hold of the lever, he again felt a sensation in his fingers of the same kind as before; and on Saturday, the 3rd, on touching the weight, this sensation was stronger, and more distinct; so much so, as to arrest his attention and lead him to mention it to some other workmen employed about the engine, who all handled the weight, and convinced themselves there was something about it very unusual. During the time they were thus employed, Patterson applied his finger gently to the lever, and perceived a spark. This was repeated by the whole party, and they soon found that sparks could be obtained from any part of the end of the boiler, as far as the valve upon the steam-pipe connecting the two boilers, and also from the pipe through which the sediment is blown, as already described. They observed further, that while standing in the volume of steam issuing from the joint, and touching the boiler, these sparks were always much stronger than when the boiler was touched by a person not in the current of steam. In one or two cases, according to their account, when the current of steam issuing from the joint was very strong, the person exposed to it being probably partially insulated by standing upon the dry and warm brick-work surrounding the boiler, gave strong sparks to others out of the current on bringing his hands to theirs; and once or twice they felt, under these circumstances, something like a slight electrical shock. It may be observed, that at this time the weather was exceedingly fine and dry. It was not long before the engineer of the colliery, Mr. Marshall, became acquainted with these circumstances, and his first feeling was to apprehend that the boiler was in danger of exploding, for, as he said, "when there was fire on the outside of the boiler, he did not know what there might be within." He accordingly sent to Messrs. Hawks's, of Gateshead, who built the boiler, for a person to examine it, and Mr. Golightly, their manager in that department, went out on Wednesday, the 7th inst., for that purpose. He gave his opinion as to the safety of the boiler, and returned much surprised at the phenomena it presented. The singular circumstance of a steam-boiler yielding electrical sparks, and giving shocks, now began to be noised abroad; and my friend, Mr. Henry Smith, of Newcastle, who had heard the account

both from Mr. Golightly and Mr. Marshall, wrote me a note acquainting me with the matter, and desiring me to go with him to see it, which I did on the 11th inst., and again on the following day, having with us the second time proper electrical apparatus. On our first visit, the boilers being unplugged and empty, we merely satisfied ourselves as to all the particulars of their setting, etc., already detailed. Next day, on our arrival, we found the engine at work, the steam up to a pressure of thirty-five pounds an inch, and blowing off strongly at the joint in the boiler. The day was a little damp, but yet not unfavourable, and we were informed on alighting that the indications of electricity were very faint and weak; however, we proceeded to our examination, of which the following is the result.—

1. On touching the boiler with the blunt point of a penknife anywhere about the circular end, the weight or the safety-valve itself, with the steam strongly blowing out of the joint, but with no part of the person exposed to the volume of steam, no spark could be perceived whatever.

2. On immersing one hand in the current of steam, and touching the parts of the boiler already named with the point of a penknife held in the other, a very minute but distinct spark was perceived, and this occurred equally on all parts of the boiler, or safety-valve, within reach.

3. By standing in the current of steam, so as to allow it to blow forcibly upon the person, the spark became larger; it was then one-eighth of an inch long.

4. On holding a large shovel in the current of steam with one hand, and touching the boiler with a penknife held in the other, a spark was obtained three-eighths of an inch long.

5. The cap of a gold-leaf electrometer, the bottom of which was held in the hand, was applied to the weight, the body of the operator being entirely out of the current of steam; and no divergence was produced whatever.

6. The electrometer held in the hand had its cap applied to the weight, the other hand of the operator being immersed in the current of steam: strong divergence was immediately produced.

From this it was evident that the electricity proceeded from the steam; but as the boiler-house was damp, so that insulation by glass could not well be preserved, a copper wire was attached to the shovel already mentioned, the end of which wire terminated in the engine-house, some yards distant from the boiler-house, where was placed a table. The shovel was held by Mr. Smith in the current of steam, with its edge about an inch and a half from the aperture through which the steam issued, and the wire leading away from the shovel was insulated by being attached to sticks of sealing-wax held by assistants. Mr. Smith stood on an insulating stool.

7. On touching a pith-ball electrometer, the threads of which were five inches long, with the insulated wire leading from the shovel held as mentioned, the balls diverged four inches with positive electricity.

8. The wire was attached to an insulated tin conductor, when it yielded sparks half an inch in length.

9. A pointed wire attached to this conductor exhibited the brush of light a quarter of an inch long, which always attends the escape of positive electricity from a point into the air.

10. A small jar was now charged so strongly as to give a rather disagreeable shock. By this time a large crowd of men, women and boys from the "Pit Raw," or pitmen's residences near the colliery, attracted by the novelty and singularity of the circumstances, had gathered about us, filling the engine-house and looking on with great curiosity and interest. A circle of sixteen of these men and women was formed, and they received together, much to their surprise and merriment, a powerful shock from the charged jar. This was several times repeated, the numbers receiving the shock varying each time from twelve to twenty.

11. A stout card was perforated by a discharge of the jar ; and cotton wrapped round the end of a copper wire and dipped in pounded resin, readily set on fire.

12. When the edge of the shovel was made to approach the aperture through which the steam issued as near as three-quarters of an inch, very vivid and bright sparks of that length passed continually between it and the boiler.

13. The second boiler did not discharge steam through any fissure, but on lifting its valve by the hand it blew off in a strong current. When the shovel was held in one hand in this current of steam issuing from the safety-valve, and the boiler was touched with a penknife held in the other, a spark passed exactly, as under the same circumstances in the boiler subjected to the above experiments.

From this it would appear that the steam of both boilers was in the same electrical condition.

During the whole of these experiments the engine was doing its work as usual, occasionally going and occasionally standing ; but no difference was observed in the electricity given off by the steam.

I have been most careful to supply an exact account of the facts of this extraordinary, and, as far as I know, unprecedented case, but I do not offer any theory to account for the phenomena. It is hardly possible to suppose that there is any local peculiarity about these boilers, or the place where they are situated, to occasion the highly electrical condition of the steam produced in them ; and yet it is as difficult to suppose the fact of high-pressure steam being

electrical, a general one ; for if it were so, it could hardly, up to this time, have escaped observation. The conditions, therefore, under which steam becomes electrical require to be investigated, and it is not unlikely that the investigation may lead to important results.

I am, Gentlemen,

Your obedient Servant,

H. L. PATTINSON.

*Bentham Grove, Galeshead,
October 19, 1840.*

Ibid.

LVII.—*Specification of a patent for an Improvement in Manufacturing White Lead.* Granted to SMITH GARDNER, city of New York, August 28, 1840.

To all whom it may concern: Be it known, that I, Smith Gardner, of the city of New York, in the state of New York, have invented an improvement in the process of manufacturing white lead, known to the chemist under the name of carbonate of lead ; and I do hereby declare that the following is a full and exact description thereof.

The first part of my procedure consists in the treating of metallic lead by the well known process by which a pulpy substance is produced, which is known to manufacturers under the name of suboxide of lead. This process consists in the placing of granulated lead, or lead in fragments, in vessels lined with sheet lead, and containing water. These vessels may be in a cylindrical form, and made to revolve on their axes, like barrel churns, or they may have a reciprocating instead of a revolving motion ; and they may be, and have been, varied in form in different ways, the only essential point in their construction being that the lead contained within them may be subject to continued attrition. Thus far, the process is identical with that which has been adopted and followed in many manufactories, in which it has been attempted to manufacture white lead from the suboxide of lead so produced.

In these attempts it has been proposed to carbonate the suboxide of lead, by putting portions of carbonate of potash, carbonate of soda, or other carbonates, into the water with the lead undergoing attrition, it having been supposed that the alkaline carbonate would give up its carbonic acid to the oxide of lead, as said oxide was formed. Independently of the known affinities of the respective articles named, I have proved, by repeated experiments, on a large scale, that carbonate of lead cannot be produced in that way. Another attempt to convert the suboxide of lead, obtained by trituration, into white lead, has been by taking the said pulpy oxide,

agitating it in a vessel containing water, and forcing a stream of carbonic acid, or of carbonic acid mixed with atmospheric air, through it. By this process a carbonate of lead has been produced, but in so imperfect a manner, as to leave it destitute of all the essential properties of that article; wanting the density, body, and freedom from colour, found in good white lead. In consequence of these defects, the attempts hitherto made to manufacture white lead from the suboxide produced by triturating fragments of lead in leaden vessels, under water, have proved abortive; but, by a very simple variation of the process, I have succeeded in producing good white lead, which has been pronounced by judges to be equal to the best that is imported.

As it was fairly proved that the suboxide would not combine with the carbonic acid, after said suboxide had been fully formed, I determined to vary the process so as to present the carbonic acid, in conjunction with a portion of atmospheric air, to the suboxide of lead in its nascent state; and this I have found perfectly effectual. In order to effect it, I triturate my lead with water in leaden cylinders, or other vessels, as above described, but, instead of leaving the vessels open, or perforating them, for the admission of atmospheric air, I make them close, by means of suitable shutters, or stoppers, which may be removed whenever it is necessary so to do; and during the whole time that the trituration is continued, I introduce carbonic acid, accompanied by atmospheric air, into the triturating vessels. When these vessels are in the form of horizontal cylinders, I pass the gases into them through hollow gudgeons; a mode of construction and procedure well known to machinists; under other forms or modes of constructing my triturating vessels, I adopt whatever means I may consider the best for introducing the gases within them. The result of this process is, that the nascent suboxide of lead presented to the oxygen of the atmospheric air, and to the carbonic acid, combines with them, and at once produces a perfect carbonate of lead, possessing all the essential properties of that article. I in general open each triturating vessel once in about twelve hours, to remove the carbonate of lead which has been formed within it. This may be done more or less frequently, according to circumstances.

When the carbonate of lead thus manufactured, is first obtained, it generally has a light tinge of blue, but this disappears in the process of drying, and it is not important, therefore, to adopt means to prevent it; I have found, however, that by introducing a very small portion of the vapour of vinegar in conjunction with the atmospheric air and carbonic acid, the white lead is at once obtained perfectly free from colour.

The carbonic acid may be generated by the combustion of coal, or by the decomposition of carbonate of lime, or of other carbonates.

Having thus fully shown the manner in which I conduct the process of manufacturing white lead, or carbonate of lead, and pointed out the difference in the process as adopted by me from those heretofore followed, what I claim therein as of my invention, and desire to secure by letters patent, is, simply, the introduction of carbonic acid and of atmospheric air into closed *vessels*, in which fragments of granulated lead is subjected to long continued attrition in water; the introduction of these gases being intended to supply the portion of oxygen and of carbonic acid necessary to convert the nascent suboxide of lead into white lead; by which means a perfect combination is effected, and the desired result attained, as herein set forth.

SMITH GARDNER.

Specification of a patent for Manufacturing Carbonate and other Salts of Lead. Granted to HOMER HOLLAND, Westfield, Massachusetts, November 3d, 1838.

To all to whom these presents shall come: Be it known, that I, Homer Holland, of the town of Westfield, in the county of Hampden, and state of Massachusetts, have invented several new improvements in processes for compounding, making, and producing pulpy compounds from Metallic lead, and of converting said pulpy lead into sulphate and carbonate of lead for white pigments; and also for making of said pulpy lead into chromate of lead, known as chromic yellow; which special improvements in compounding have not heretofore been known or used: and that the following is a full discriminating, and exact description of said methods, sufficient in detail to distinguish the same from all other processes, and to enable any one skilled in chemistry to apply and use said improvements understandingly. The special improvements which I would describe and claim, consist, 1st. In using any alkaline salt, or substitute, in the moistening solution for the charge and chamber, or open headed cylinders, described and mentioned in my patent dated the 18th day of March, 1836, whose elements consist essentially of oxygen, carbon, and hydrogen, in any proportions, instead of alkaline carbonates, before recommended and employed, as they augment the electro-chemical action, increase the product, and modify and facilitate the combination of the elements with nascent pulpy lead, by their presence, or catalytically.

Acetates of lead, whether neutral or basic, also sugar, and even alcohol, may be advantageously used in the solution, to moisten charge, chamber, and pulp.

2d. In adjusting the pulpy plumbic compound, produced as described in my said patent, for acetate and nitrate of lead, or with

the catalytic additions with neutral chromate of potash or soda, or by dissolving the alkaline chromates in water, and using this chromic solution as the moistening of charge and chamber.

The chronic pulp, after subsiding, may have most of all the alkali withdrawn by decantation, and the remainder neutralized by washing with water, made acid by sulphuric or other acid.

The commercial bichromates of potash and soda are to be made neutral by the addition of suitable proportions of their respective bases.

The economy of the above process, in making chromate of lead, is in substituting the plumbic compounds in their nascent state, for the expensive plumbic salts, acetate and nitrate, now usually employed in the manufacture of chromic yellow.

3d. In my said patent for oxidizing and producing lead pulp, although, in the incipient stage of the operation, the lead may be seen under oxide, the subsequent exposure, in the open-headed chambers, to the continuous and conjoint action of the elements which constitutes the atmosphere, water, and catalytic additions, together with the friction, and the known and established property, or capacity, which all metals, in a minute state of division, have of absorbing, "dissolving," or combining with, all elements with which they are in contact, constrains me to disclaim the opinion, that plumbic pulp, under any circumstances, can be considered a definite compound, and much less an oxide; but that it is a compound of lead, into which the elements, hydrogen, carbon, and nitrogen, and their compounds, enter, as well as oxygen.

By the foregoing explication of the pulpy plumbic compound, the following rationale of the modifications of the pulp, in converting it into a perfect carbonate, or sulphate, will be apparent. After carbonating the pulp with certain catalytic additions, artificially, should there be any basic salt, it is to be removed by washing in an alkaline solution, boiling, particularly in making the sulphate of lead, the pulp must be boiled to modify the plumbic hydrate by more highly oxydizing the pulp.

The sulphate of lead is made directly from the pulpy lead, modified and oxydized by heat, while in its moistened state, by digesting it, in any quantity, with sulphuric acid of commerce, previously diluted with twice its measure of water, (more or less,) and suffering the acid thus diluted to become perfectly cold, previous to adding the pulpy lead.

It is necessary to boil the dilute sulphuric acid and pulp thoroughly together in a shallow leaden vessel, with rather an excess of acid, that the product may become a perfect sulphate; in this, great caution is requisite, otherwise the product will be, more or less, a mixture of sulphate, hypo-sulphate, or sulphanide of lead,

and its colour changed by mixing and painting in oil. Besides, it will not be as dense, fine, and fusible.

All the pigments should be thoroughly washed in several waters, and may be dried by the well known methods.

The cylinders mentioned in said patent, I now make about four feet in length, and thirty inches in diameter, wholly of lead, either sheet or cast, about one-fifth of an inch in thickness. The ends are entirely open, except an inner rim to retain charge and moistening fluids, or solution, with forming pulp, and allow a free circulation of the atmosphere for its elements.

They are mounted on an axis, passing through their centres, and the centres are of iron, with arms which are attached to the rims of each end of the cylinders. The rotations may vary from six to nine times a minute, and are moved by a drum and belt, or other gearing. The pulpy lead may be withdrawn every six, eight, or twelve hours. The medium charge is fifty pounds, and the moistening fluid, or solution, from three pints to three quarts, or more.

I claim, 1st. The process and method of using the alkaline salt, carbonates, and other catalytic substitutes, as hereinbefore mentioned, in moistening charge, and chambers, described and mentioned in said patent, in producing pulpy plumbic compounds; and I do not intend to restrict their application and use to pulpy leads produced by revolving chambers alone, but to extend their application to the compounds of lead produced by other methods of friction, whether substituted, or adopted, to evade my chambers.

2d. I claim making chromate of lead, as above specified and described.

3d. I claim modifying the pulpy plumbic compounds above described for carbonate of lead, and particularly the processes described for making a definite sulphate of lead, by digesting, boiling, and washing, as above discriminated, and made plain and distinct.

HOMER HOLLAND.

Remarks by the Editor.—We have inserted the three foregoing specifications on the manufacturing of white lead, and of other compounds of lead, because the particular process upon which they are dependent, that of producing these compounds from lead comminuted by trituration, has, of late, excited much interest, and been a subject of frequent inquiry. The first of these specifications leads to the conclusion, that Mr. Holland supposed this process to be new in the year 1836, whilst the fact is that it was the subject of a patent obtained by Joseph Richards, of Philadelphia, in the year 1818. A manufactory was also established at Norristown, Pennsylvania, in which the trituration process was employed, and after assaying the thing for a considerable length of

time, the plan was given up. The white lead produced was deficient in body, and its colour was said not to be good.

That Mr. Holland found the process of 1836 defective, is to be inferred from his patent of 1838, for improvements in it. We should be glad, however, to obtain his own account of this matter, as we might err greatly by detailing the information received from others.

Mr. Holland's second specification we think much more elaborate than clear; had language more simple been used, it would have rendered his meaning more obvious to the great body, even of those "skilled in the art." We have ventured to insert, and to change a few words, where we thought that it might be safely done, but further than this we have not gone.

On the 7th of June, 1838, Mr. William Cumberland, of New York, obtained a patent for a process of manufacturing a white pigment, the specification of which we published in vol. xxiii. p. 402. The patent obtained by Mr. Gardner is, it will be seen, for a particular variation of the process of oxidizing and carbonating the pulpy lead, and by which, he states, a very superior white lead is obtained; and his statement has been corroborated by others. We shall have something further to say on this subject.

Journal of the Franklin Institute.

LVIII.—*On the course or path of the Electric Fluid.* BY HENRY DIRCKS, ESQ.

Read before the Literary and Philosophical Society of Liverpool, Nov. 16.

Although we have two principal theories by either of which we may account for electrical phenomena, yet as is well known there is no theory that is universally adopted. We prefer that of Du Fay of two fluids, the resinous and vitreous, whereas in America the Franklinian theory of a single fluid continues to be received. It is certainly a curious and remarkable fact, that this important point which appears to be at the very head of our inquiry, in investigating the nature of this exceedingly subtle agent, should have so long withstood every effort that has been made to develop its operation; and that with our expended means of pursuing this interesting investigation, philosophers should still remain divided in opinion. We agree that it is the same agent which is at work in atmospheric, frictional, magnetic, voltaic, organic and thermo-electricity. The same data are taken up by the favourers of either

theory to prove their several positions ; that influence of *points* is alike advanced to prove the existence of one and of two fluids. Franklin and all electricians after him, speak of the *star* and the *brush*, the former negative, the latter positive ; whereas Dr. Faraday contends that under favourable circumstances, and especially in some gases, the negative and positive points both offered the electric brush of light. We are all familiar with the experiments proposed as evidence of the existence of a single fluid, as the action of a flame between two balls, one positively, the other negatively electrified, by which the latter becomes very much heated ; the stream of air produced when a point proceeds from a conductor ; the manner of charging the Leyden phial ; and especially that given by Mr. Lullin, when the discharge of a jar is made to perforate a varnished card, between two points on either side, but half an inch asunder, by which the point proceeding from the negative side invariably perforates the card, although a hole may have previously been made opposite the positive point, where a perforation does not otherwise occur ; also the common discharge through a card placed against a charged jar, where a burr is produced on both sides, but more markedly if the card is set vertically between the points of the universal discharger, when the burr will be found larger on the negative side, where the positive electricity may be supposed to make its exit, and smaller on the positive side, the outlet for the negative or resinous electricity. The appearance by perforating bodies, might at first seem conclusive that there are two fluids, but it has occurred to the writer, and may be worthy of notice here, in explanation of the double burr, though he has never met with any notice of a similar view of this subject, being taken by others, that, as the electric fluid is so rapidly excited by friction, pressure, and slighter causes ; the electric discharge itself, by its amazing rapidity, may become the exciter of a quantity of the fluid previously latent, which brought into activity, a reaction may be thereby produced, and this whether there is one or two fluids. This seems to be both a reasonable and highly probable consequence.

We here have instances of the effects of the electric fluid, but can neither arrive at any conclusion respecting its nature, nor ascertain the direction of its course. As we might hope to arrive at something more conclusive by considering this latter point, which indeed is the main object of the present paper, we shall proceed to this more important inquiry.

One known means of tracing the passage of the electric discharge is that made when the points of the universal discharger are placed an inch apart on a card, having a broad line painted on it with vermilion, when the discharge leaves a well defined irregular black line. Observation in this way, however, is very limited. We wish to arrive, for instance, at something definite whether there is one or two fluids—and we wish to see in the path it takes

whether it passes right over, meets half way, or passes side by side. In short, what are the peculiarities exhibited by the discharge of the Leyden jar?

Dr. Faraday, in his most excellent and elaborate "Researches," states that an ever present question on his mind has been, "Whether electricity has an actual and independent existence as a fluid or fluids, or was a mere power of matter, like that we conceive of the attraction of gravitation. If determined *either way*," he adds, "it would be an enormous advance in our knowledge." Not only every experiment which has for its object the elucidation of electrical phenomena, but likewise the opinions of electricians may truly be said to be of extreme value. It is well, therefore, that Dr. Faraday has put on record as well in what he succeeded as in what he failed. The ill success of one may suggest another course of experimental inquiry to some other worker in this prolific field of scientific research, and thus we may hope gradually to develop many important results in connexion with electrical science, from which, with good cause, we expect to reap many discoveries of great practical benefits.

It early appeared to me quite within the range of possibility to render this active fluid a tell-tale, as it were, of its own progress, especially in conducting the discharge of the Leyden battery. I felt convinced of this from what has already been noticed of the piercing of cards, the black line left on a vermillion coloured card, and also from the markings left on the uncoated glass by the spontaneous discharge of an overcharged jar. But my object was to obtain evidence on a larger scale, and of a more conspicuous character.

My first experiments were made with a piece of window glass four inches square, smeared on one side with a mixture of flowers of sulphur and white lead ground together with gum water, laid evenly on the glass and dried. When placed against the side of the Leyden jar, the charge may be passed over it by using the discharging rod, in which way a dark brownish line two or three inches long, having a circuitous course, is easily produced.

Not satisfied with this result I at length adopted a plan which successfully affords an interesting illustration of the path of the electric fluid through a considerable space, varying with the quantity of charged coated surface. From 18 inches to 2 feet is easily obtained with a gallon jar, or battery of equal capacity, provided the electrical machine is in good working order. The means of effecting this will appear very simple, though the conditions requisite for its success are not so obvious as might at first appear. Take a broad oblong plate of glass, place under it a sheet of white paper, then by striking a fine hair-sieve containing iron filings, let fall on the glass an equal distribution of the filings until they communicate a dark-grey shade over the paper. The glass so prepared

is to be placed in the line of communication for making the discharge. When this is done with the white paper under the glass, the result is most conspicuous, beautiful, and interesting. The appearance that instantly follows is something like a map of a serpentine river, with often small branches issuing out in many streams at some of its principal windings and again running into the main branch. Throughout the tortuous course of this passage the iron filings are swept away to the breadth of one-eighth to a quarter of an inch and upwards by the rapid transit of the fluid, with as much neatness and precision as if carefully removed by some process requiring extreme care and delicacy of manipulation. Often a few grains form an irregular central line. If a short piece of crooked wire in the form of a ring, arch, or helix, be placed in or a little out of the direction of the fluid, it is made part of the circuit, and the filings are not disturbed if any arched form or immediate connection offers a more perfect conductor. On shaking the filings off the glass no trace appears to remain, until breathed upon, when a clear thread like line, having a slight dark colour, becomes distinctly observable.*

The success of this experiment seems to depend on a peculiar arrangement, and the best I have found, is to have the Leyden jar placed on the edge, and touching the filings at one end of the glass plate; a perpendicular rod of thick wire being at the other, from the top of which, a connexion may be made (by a discharging rod,) with the ball of a Leyden phial. A full charge is requisite to make a good marking of the path, and the filings should not be too thickly spread, otherwise the electricity passes over in flashes; a communication, too, should be made between the outside of the jar, and some good conductor. The vertical pillar at the further end of the plate, has been formed to answer when long thin bent wires proved quite ineffectual.

It is only to be regretted that this beautiful experiment leaves the subject still open to enquiry; but this may be one step, which, in other hands, may be made serviceable in obtaining greater results. I cannot pass over, in this place, mentioning a very easy means of tracing, and so registering, the several experiments made at each discharge. This is done by taking the glass, strewed with filings, and having a marking which is to be copied; on each end or down each side place a thin lath, on this lay another, but of plain glass of equal size, over all place a slip of paper long enough for a tracing. Now, rest the glasses between two tables set apart, or between two flat bars of wood, resting on a table, and in such a situation, that a small lighted candle placed on the floor, will throw the shadow of the filings up through the glass on the back of the paper. There being no other light in the room, this is easily done. Or by giving a coating of thick glue to cartridge paper; this, if carefully managed, would take up the filings off the glass, and show a reversed

* A description of the apparatus will be seen at the end of the article.

specimen of the electric path. In this way I have taken the filings and preserved the figure made by the magnet.

Another experiment, too interesting to be omitted, was performed with a few sheets of strong printing paper, stitched like a pamphlet. In the first experiment made with this, it was laid on the table of the universal discharger, and the balls being removed, the blunt pointed wires were placed on the paper, an inch and a quarter asunder; the discharge of a very large jar, slit the paper, giving it the form of two small folding doors. With a mixture of equal parts of flowers of sulphur, and red lead, the face of the upper and three lower leaves were strewn over. The result on making the discharge was not always the same—thus

Ex. 1. In a passage of one and a quarter inch, the positive end was harmlessly passed over for more than one-third, leaving only a dark line on the top leaf; from hence to the negative end, the paper was ripped open, the cut being in shape like the letter H. On examining the lower or second leaf, the remaining two-thirds of the passage, that is, the horizontal line of the H., presented a broad black marking, which had struck also to the under side of the upper leaf. The third leaf was untouched.

Ex. 2. This was precisely the same as the foregoing, with the exception of being a shorter path, and more violently torn, so that the rent, formed a very oblong H., and the positive side was uninjured for near half way. The remaining half, which we call the negative side, showed a broad black band on the face of the second leaf.

Ex. 3. This passage was remarkable from the paper being pierced on the positive side, clear of the rent beyond it, which was of a very imperfect H form. The paper was unmarked and uninjured for a quarter of the path on the positive side, at the end of this the paper was pierced with a small hole. On the second leaf, a round black spot occurred, corresponding with this terminus of the positive side, and at the negative end where the rent begins, there was another black spot or star, both connected by a straight cut in the paper not discoloured, and branching off right and left at the negative end, in form like a T. The third leaf not marked.

Ex. 4. Here the passage from the positive was marked one-third with a faint line, at the end of which a small hole appears, and another hole at the commencement of the negative passage, without tearing the paper. On the second leaf these holes have corresponding black perforated spots, and on the third leaf there is a broad black mark, with a corresponding one on the upper side of the leaf above it. These black marks are all very like the representation of mountains in a map, and have a white band running through their centre.

Here, as in Mr. Lullin's experiment, there is a tendency on the negative side to enter the paper, although its surface is covered

with a conducting substance. There is more violence, too, on this side, where indeed we have a disruptive discharge. These experiments are on many accounts exceedingly interesting. It would appear as if the positive or vitrious electricity had greater velocity than the negative, that the two electricities meet at this point, and uniting cause an explosion, followed in this instance by a chemical effect—the production of a sulphuret of lead, which marks *only* the remaining two-thirds of the path. This, if correct, would seem to offer some modification in the remarks Dr. Faraday makes on the current—he says, “It is a most important part of the character of the current, and essentially connected with its very nature, that it is always the same. The two forces are *every where* in it. There is *never* one current of force, or one fluid only. Any one part of the current may, as respects the presence of the two forces there, be considered as precisely the same with any part; and the numerous experiments which imply their possible separation, as well as the theoretical expressions which, being used daily, assume it, are, I think, in contradiction with facts.” What he next adds is too remarkable in connexion with our experiments not to call for special notice. “It appears to me to be as impossible to assume a current of positive or a current of negative force alone, *or of the two at once with any predominance of the one over the other*, as it is to give an absolute charge to matter.” (1627.) The establishment of this as a fact or its disproof he justly considers of the utmost importance.

We might almost be inclined to inquire in reference to the electrical experiment from the consideration of which we have digressed. Has the resinous electricity a tendency downwards, and the vitreous a tendency upwards? Or, has the latter greater velocity than the former? Or do these experiments at all prove “that the centres of the two forces (or electricities), or elements of force, can be separated to any sensible distance?”

November, 1840.

Fig. 2, plate vii., Shows the arrangement of apparatus for making a long path through iron filings. A, the Leyden jar; B, a glass pillar mounted with a wire supporting the metal bar C, D. A, D, B, a plate of glass strewed over with iron filings. C, E, F, the discharging rod by which to complete the connexion in making the discharge.

Fig. 3. A, B, D, the glass plate and filings displaying the path of the electric fluid. A, the negative end, B, the positive.

LIX.—On Electro-Magnetic Forces. By J. P. JOULE, Esq.

67. I have in my last paper described a method of constructing the electro-magnet which is attended by great results. The few additional experiments which I have now the pleasure of submitting to the readers of the “Annals,” are, I think, confirmatory of the principles before advanced.

68. A piece of *stub* iron was (as in the manufacture of gun bar-

rels) formed into a spiral and welded on a mandril into the shape of a thick tube, by which process the iron was rendered very compact and sound throughout. This, and another piece of iron which was intended for an armature, were planed, turned, and fitted with eye hole screws in the manner that I have previously described (39)*. In fig. 1, pl. vii. C. represents the electro-magnet, D. the armature, and A. B. a conductor of copper rod or wire passing along one side, returning by the axis, and then away by the other side, so as to go about the whole once only, and in a shape somewhat similar to that of the letter S. The length of the electro-magnetic cylinder is two feet; its external diameter is 1.42 in., and its internal, 0.5 in.; the weight of the iron of the magnet, with the screws, is 6 lbs. 11 oz.; that of the armature 3 lbs. 7 oz.; and the least sectional area of the magnetic circuit (49), $10\frac{1}{2}$ square inches. This electro-magnet, in order to distinguish it from the rest, I call No. 5.

69. A copper rod, $\frac{3}{4}$ of an inch thick, was covered with a ribbon of cotton, and bent about the cylinder as I have just described. The electro-magnet and its armature were then secured, by means of cords passing through the eyeholes, to strong pieces of iron affixed to the *levers* (43). A battery consisting of eight of the cast iron cells (66), each of which presented an effective surface of two square feet, was arranged as a single pair, and, in connection with the electro-magnet, induced a lifting power of about 1350 lbs.

70. Being aware that a bundle of thin wire is a much better conductor than a rod of the same weight and length, I removed the copper rod and substituted for it a bundle, consisting of 60 copper wires, each 1-25th of an inch thick. With this arrangement it was found that 16 cast iron batteries, in a series of 2, produced a lifting power of 1856 lbs., or 183 times the weight of iron employed in both the magnet and its armature.

71. Now, by dividing the power thus obtained by the least sectional area of the magnetic circuit upon which it is induced, we have a specific power of 181, which is only two-thirds of that which a comparison with other electro-magnets would lead us to expect.† This deficiency is, I think, mainly owing to the very small relative quantity of conducting metal about this (No. 5) electro-magnet, a deficiency which demands a proportionate increase of battery power, in order to produce the same effects. This, with the difficulty of making the weight bear evenly on every part of so long a cylinder, may, I think, satisfy us that, if every circumstance were strictly attended to, its maximum lifting power would obey the general rule.

72. Having suspected that the extreme power of the large electro-magnet, No. 1‡, had not been attained in my last experiments, on

* Annals for September, Vol. 5, p. 190.

† See Table 4, Annals, Vol. 5, p. 193.

‡ For a description of this electro-magnet, see (39), Vol. 5, p. 190. It has

account of the imperfect insulation of its coils (45), I was determined to try it again, and to use every precaution which was calculated to develop its magnetism to the full extent. The old wire was removed, and 21 copper wires 1-25th of an inch thick, and 23 feet long, were bound together by cotton tape. This was wrapped on the iron, which had been previously insulated by a piece of calico.

73. Sixteen cast-iron cells, of the same size as those previously described, were then arranged in a series of four, and connected by sufficiently good conductors to the electro-magnet. The power which was then necessary to break it from its armature was 2775lbs., or nearly a *ton and a quarter*. An immense weight, when it is considered that the whole apparatus, magnet, armature, and coils, weighs less than 26lbs.

74. Now by the formula $x = 280 a$ (51), we have $280 \times 10 = 2800$ for the greatest lifting power of this electro-magnet, or only 25lbs. more than that actually found, which cannot but be considered as a striking proof of the accuracy of the general principles I have before advanced (49). That the *saturation* of the iron was very nearly effected, appears from the fact that the quantity of electricity used above, was fourtimes as great as that which was competent to make the same electro-magnet carry 19 cwt.

75. Although the battery that I have used for obtaining *maximum* effects is very powerful, a very good lifting power may be attained by means of a very small voltaic arrangement. For instance, No. 1 can carry 8 cwt. when the current generated by a single pair of 4-inch plates of iron and amalgamated zinc, is passed through its coils; and with single plates of platinized silver and amalgamated zinc, exposing only two square inches of surface, the attraction is such as to require the utmost force I can exert, even to *slide* the armature.

Broom Hill, near Manchester, November, 23rd, 1840.

Errata in Mr. Joule's paper on Electro-Magnetic Forces.— Vol. 4, p. 478, table 4, for 16·6 read 1·66. Vol. 4, p. 481, line 25, for considerable read considerably. Vol. 5, p. 196, table vii., for 26 and 11 read 2·6 and 1·1.

been presented to the "Royal Victoria Gallery" of Manchester, where it still remains on exhibition.

BRITISH ASSOCIATION PROCEEDINGS AT GLASGOW, 1840.

Dr. Playfair "On a New Fat Acid."—Dr. Playfair had examined some of the vegetable fats, for the purpose of ascertaining whether the margaric acid contained in them possessed a constant composition. He remarked that the acid in the butter of nutmegs was peculiar, and had not formerly been examined. Pelouze and Bondet have stated in the *Annales de Chimie*, that it is margaric acid. Dr. Playfair considered that the radicals of sercic and œnanthic acid were similar; in the former, however, one equivalent of hydrogen is replaced by one equivalent of oxygen. It is a beautiful white crystalline compound melting at 49° C., and is soluble both in alcohol and ether. The combination of the acid with oxide of glyceril, exists in the butter; it unites with metallic oxides and forms salts: these were described, but the results are not susceptible of analysis, as they were principally numerical. The formula of the acid is $\overset{28}{C} \overset{54}{H} \overset{3}{O}$.

Dr. Ettling "On the Identity of Spiroilous and Saliculous Acid."—The oil discovered by M. Pagenstecher, and obtained by the distillation of the *spiræa ulmaria*, has already attracted considerable attention. Dr. Ettling had analyzed it previously to the appearance of M. Piria's valuable paper on Salicyl. The oil decomposes into two oils on keeping, one of which is specifically lighter, the other heavier than water. Dr. Ettling discovered that the latter possessed the same composition as hydrated benzoic acid. The action of ammonia on the oil gives rise to some new interesting compounds. In order to obtain these compounds it is indifferent whether saliculous or spiroilous acid be employed. The final product of the action of ammonia upon these, is the amide of salicyl (salicylamide). This body evidently belongs to the class of amides, for it does not evolve ammonia, on the addition either of potash or of acids. The cause of its formation is as follows: three atoms of saliculous acid unite with three atoms of ammonia, and form saliculite of ammonia, whilst three of hydrogen and oxygen combine together and form water. This salicylamide unites with copper, iron, and lead, forming compounds.

Professor Liebig "On Poisons, Contagions, and Miasms."—Dr. Playfair stated that he had prepared, at the request of the author, a statement of Professor Liebig's new views on the subject of poisons. Poisons might be divided into two classes, those belonging to the inorganic and organic kingdoms. Many substances were called inorganic poisons which had in reality no claim to be considered as such. Sulphuric, nitric, and muriatic acid, when brought into contact with the

animal economy, merely destroyed the continuity of the organs, and might be compared, in their *modus operandi*, to the action of a heated iron, or a sharp knife. But there are others—and these are the true inorganic poisons—which entered into combination with the substance of the organs without effecting any visible lesion of them: Thus it is known, that when arsenious acid or corrosive sublimate is added to a solution of muscular fibre, cellular tissue, or fibrin, these enter into combination with them, and become insoluble; when they are introduced into the animal organism the same circumstance must happen. But the bodies formed by the union of such poisons with animal substances are incapable of putrefaction; they are incapable, therefore, of effecting and suffering changes; in other words, organic life is destroyed. The high atomic weight of animal substances explains the cause of such small quantities being requisite for producing deadly effects. After stating several chemical details on this subject, it was shown that to unite with 100 grains of fibrin, as it exists in the human body, (in which it is combined with 30,000 parts of water) only $3\frac{1}{2}$ grains of arsenious acid are necessary, or 5 grains of corrosive sublimate. The second class of poisons were those belonging to the organic kingdom. For some such substances as brucia and strychnia, no data exist by which it can be determined to what cause their action may be assigned. But the morbid poisons, such as putrid animal and contagious matter, appear to owe their action to a peculiar agent, which exerts a much more general and powerful action than chemists are aware of. Thus, when oxide of silver is thrown into peroxide of hydrogen, the oxide is reduced, and metallic silver remains. Here there can be no affinity, for oxygen can have no affinity for oxygen. It is merely that a body in a state of motion or decomposition is capable of inducing upon or imparting its own state of motion or decomposition to any body with which it may be in contact. There is a disease frequently produced in Germany by using decayed sausages as an article of food. The symptoms attending the disease are remarkable, and distinctly indicate its cause. The patient afflicted with the disease becomes much emaciated, dries to a complete mummy, and finally dies. The muscular fibre and all parts similarly composed disappear. The cause of this evidently is, that the state of decomposition, in which the component parts of the sausages are, is communicated to the constituents of the blood, and this state not being subdued by the vital principle, the disease proceeds until death ensues. It is remarkable that the carcasses of the individuals, who have died in consequence of it, are not subject to putrefaction. The cause of the action of contagious matter is similar. It is merely a gaseous matter in the state of transformation, and capable of imparting the state of transposition, in which its atoms are, to the elements of the blood. It is capable of being reproduced in the blood just as yeast causes its own reproduction in fermenting wort. The causes of the action of yeast and of contagion were shown to be the same, and examples were produced in which similar reproductions take place in common chemical processes. There are two kinds of yeast used in the brewing of Bavarian beer.

The fermentation caused by one is tumultuous ; that produced by the other is tranquil. They, therefore, induce the peculiar state of transposition in which their atoms are upon the elements of the sugar. The same was shown to be the case with the vaccine virus of cow and human small-pox, of which, one produces a violent action upon the constituents of the blood, whilst the other causes a gentle action quite distinct from the former.

Professor Hannay said he could not exactly coincide with the views proposed regarding the action of inorganic poisons, as he was convinced the cause of their virulence was owing to something further than mere combination with the animal membranes ; nor could he coincide in the comparison brought forward by Dr. Playfair, that sulphuric and oxalic acids merely acted like a heated iron, by destroying the continuity of particular organs. He thought that through the course of the inquiry chemistry had been too much kept in view, and that medicine had not been sufficiently consulted. It was singular to see us brought back to the time of Hippocrates, who also had affirmed that contagious matter was a kind of yeast acting in the blood. Dr. Playfair explained that Professor Liebig expressly states in his report, that this subject cannot be completed without the co-operation of physiologists ; that he had therefore merely brought forward the purely chemical part of the inquiry, and hoped thereby to draw the attention of physiologists to its further investigation. Hippocrates had certainly compared the action of yeast with that of contagious matter, and the comparison was so apt that it could scarcely be avoided ; but the merit of Liebig's views is, that he has explained the action of yeast, and shown that it is owing to a peculiar agent which has hitherto escaped attention, but which plays a very important part in the phenomena of combination and decomposition.

PROF. FORBES IN THE CHAIR.

On the Decomposition of Glass, BY SIR DAVID BREWSTER.

There is no subject more curious or more instructive than the disintegration of crystallized and uncrystallized bodies, either by the direct influence of chemical agents, or the slow process of natural decomposition. At the Edinburgh Meeting of the Association, I submitted (said Sir David) to this Section a brief account (which has been since published in an enlarged form in the Edinburgh Transactions) of remarkable optical phenomena produced by the instantaneous action of water and other fluids on crystals, and on their subsequent decomposition when placed in their saturated solutions. Since that time I have had occasion to examine the phenomena of decomposed glass, both of that which is found in Italy, of which I have received the finest specimens from Mrs. Buckland and the Marquis of Northampton, and of other specimens recently found in making excavations among the ruins of the Chapter-house of the Cathedral of St. Andrew's. In decomposed glass, the decomposition commences in points, and

extends itself either in planes so as to form thin films, or in concentric coats so as to form concentric films. When the centres of decomposition are near each other, the concentric films or strata which they form interfere with each other, or rather unite, and the effect of this is, that the glass is decomposed in film of considerable irregularity, their surfaces having a finely mammillated appearance, convex on one side and concave on the other. The films thus formed are of extreme beauty, and afford, by transmitted light, colours of infinite beauty and variety, surpassing anything produced in works of art. They have the effect of dissecting, as it were, the compound surface of the solar spectrum, or of sifting and separating the superimposed colours, in a manner analogous to what is produced by coloured and absorbing media. I have succeeded, indeed, in producing one or more bands of white light incapable of decomposition by the prism; and there can be no doubt that they will be found to exercise a similar or an analogous action on the leading rays on the thermometric spectrum. In the decomposed glass from St. Andrews, a change of a very different kind is effected. In some cases the siliceous and metallic elements of the glass are separated in a very singular manner, the particles of silex having released themselves from the state of constraint produced by fusion and subsequent cooling, and arranged themselves circularly round the centre of decomposition; while the metallic particles, which are opaque, have done the same thing in circles alternating with the circles of the siliceous particles. This restoration of the silex to its crystalline state, is proved by its giving the colours of polarized light, and possessing an axis of double refraction.—The notice was illustrated by diagrams and specimens of the different kinds of glass referred to.

Prof. Forbes observed that few persons can form any correct conception of the total amount of the value of glass used in the various optical instruments, on the correct action of which so much depended. Whether the decomposition which Sir David Brewster had now brought under notice, arose from the action of the atmosphere, or from intermolecular action, as Sir David Brewster seemed to think, or from some original defect in making or annealing the glass, it was of the utmost consequence. Dr. Traill had given him a specimen of a piece of plate glass manufactured near Liverpool, which, when polished, proved to be filled with fissures and flaws in the interior; and he informed him that the manufacturers had no means of ascertaining the defect, which frequently occurred, until they had gone to the expence of polishing the plates.—Sir David Brewster said, that the value of the glass employed in philosophical instruments was indeed incalculable, and that the most valuable glass articles manufactured by Fraunhofer,* of Munich, seemed to be peculiarly liable to some superficial decomposition of this kind. A prism of this glass in the Observatory of Paris

* M. Lamont, the Professor of Astronomy at Munich, who is in the constant habit of using Fraunhofer's glasses, was not present at this conversation; but he afterwards informed Sir David Brewster that there was an easy and effectual remedy for this tendency of Fraunhofer's glass to deteriorate on the surface, which was, to rub it frequently with the finer parts of whiting prepared by elaborating a mass of whiting in water, the fine powder to be dried and used on old soft linen.

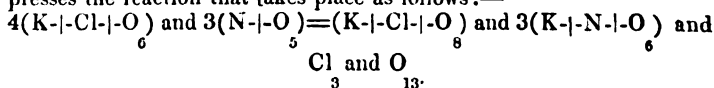
had become absolutely black. A prism belonging to himself had become quite blue on the surface, although as yet its action on light was not effected. The large object glass of the principal telescope in the Observatory of Edinburgh had begun to show decided symptoms of superficial decomposition; and many other instances also could be mentioned. He considered it of the utmost importance that a remedy should be discovered and applied.—Prof. Forbes mentioned some instances of this kind of decomposition taking place in telescopes on the continent, which showed that it was nothing peculiar to our climate.—Sir David Brewster did not think it arose from atmospheric action at all, but from some mutual action of the particles of glass themselves.—Prof. Forbes, Then why is it confined to the surface? why does it not pervade the mass of the glass?—Sir D. Brewster, Because at the surface the particles have more freedom than within; and if the new compounds are larger than the glass itself, then they have power to expand.

On the Rings of Polarized Light produced in specimens of Decomposed Glass. By Sir David Brewster.

In the course of a series of experiments “On the Connexion between the Absorption of Light and the Colours” of thin plates, published in the *Phil. Trans.* 1837, I accidentally observed under the polarizing microscope certain phenomena of polarized tints of great beauty and singularity. These tints were sometimes linear and sometimes circular, and in some specimens they formed beautiful circular rings traversed by a black cross, resembling the phenomena of mineral crystals, or those produced by rapidly cooled circular plates or cylinders of glass. Having found in the decomposed glass from St. Andrews that the siliceous particles had resumed their position as regular crystals, and arranged themselves circularly round the centre of decomposition, I was led to suppose that this was the cause of the phenomenon, and that the rings were the effect of the double refraction of the minute crystals. A few experiments, however, overturned this hypothesis, and I was soon satisfied, by a little further investigation, that the phenomena arose wholly from the polarization of the transmitted light by *refraction*, the splendid colours being entirely those of thin plates, which were sometimes arranged so as to have the appearance of concentric rings. The structure by which these effects were produced, was compared by the author to a heap of very deep watch-glasses laid one above another. When the thin films were arranged longitudinally, and were inclined to the general surface of the plate, so as to transmit the rays obliquely, the light was still polarized, but only in one plane—namely, a plane perpendicular to the plane of incidence. When a drop of *water* or *oil* was introduced between the films, the phenomena of *polarization* as well as of colour instantly disappeared. (This paper was illustrated by coloured drawings.)

On the action of Nitric Acid on the Chlorates, Iodates, and Bromates of Potassa and Soda. By Prof. F. PENNY.

The present communication contains the details and results of some experiments undertaken with the view of obtaining additional confirmation of the correctness of the author's researches on equivalent numbers. In this he has been disappointed, as the action is attended by circumstances which render it inapplicable to so delicate a purpose as the determination of equivalent numbers. The results, however, that he has obtained are new, and he considered them of sufficient interest to be worthy the attention of the Section. In order to examine the action of nitric acid upon chlorate of potassa, a known weight of the salt was mixed in a retort with a measured quantity of the acid, and the mixture heated on a sand bath; as soon as it became warm, chlorine and oxygen were evolved in a state of mixture and not of combination, and the chlorate slowly disappeared. The solution was then evaporated to dryness, and the saline residue was found to be a mixture of hyperchlorate and nitrate of potassa, in the proportion of three equivalents of the latter to one of the former. The author expresses the reaction that takes place as follows:—



The action of nitric acid on chlorate of potassa differs, then, from the action of sulphuric acid on the same salt. With nitric acid the salt is decomposed tranquilly, and the chlorine and oxygen liberated uncombined, whereas with sulphuric acid these gases are evolved in a state of combination, forming that dangerously explosive compound, chlorous acid. Nitric acid is therefore to be preferred for the preparation of hyper-chlorate of potassa, as with it the operation may be conducted without those violent detonations that are so apt to occur with sulphuric acid. The action of nitric acid on chlorate of soda is the same as upon chlorate of potassa. The chlorine and oxygen set free are in a state of mixture, and every four atoms of chlorate yield three of nitrate and one of hyper-chlorate. The hyper-chlorate of soda is a very soluble salt, and crystallizes in small rhombs. It is readily decomposed by heat, but is unacted upon by hydrochloric acid. It deliquesces by exposure to the air. The action of nitric acid on an iodate is very different from that on a chlorate, and is well illustrated in the case of iodate of potassa. When iodate of potassa is boiled for some time with a large excess of nitric acid, it is decomposed into potassa and iodic acid, the potassa combines with its proportionate quantity of nitric acid, forming the nitrate, and the iodic acid is deposited from the solution in minute, hard, and transparent crystals. If the acid solution of nitre, containing the iodic acid, be then evaporated, a reaction takes place; the iodic acid decomposes half of the nitre, sets free its nitric acid, and combines with the potassa, forming the biniodate. This change is completed when the mixture is dry, and if the heat

be then withdrawn a definite mixture of biniodate and nitrate is obtained. If the heat be continued, a still further change occurs, the iodic acid expels the whole of the nitric acid, which is evolved as nitrous acid, and oxygen and neutral iodate of potassa remain. By adding a fresh portion of nitric acid to this iodate, the same changes may be produced by a proper regulation of the temperature. By acting upon iodate of soda with nitric acid, Prof. Penny has obtained a biniodate of soda, and by adding a considerable excess of iodic acid to a solution of iodate of soda he has found a teriodate of soda. Both of these salts are anhydrous. The biniodate of potassa contains one atom of water. He also finds that crystals of iodate of soda contain different quantities of water, according to the strength of the solution from which they have deposited. From a hot and strong solution of this salt crystallizes in acicular tufts, and these crystals contain two atoms of water. If the solution be rather weak, long four-sided prisms are obtained, and these contain six atoms of water. If a solution of iodate of soda be evaporated spontaneously, large irregular prisms deposit, and these contain ten atoms of water. They effloresce rapidly by exposure to the air, and lose in this way eight atoms of water. The action of nitric acid upon bromate of potassa was next examined, and was found to differ remarkably from the actions of this acid on the chlorate and iodate. Neither hyper-bromate nor bibromate is produced, but merely nitrate of potassa. The nitric acid sets free the whole of the bromic acid, and this, at the moment of its liberation is resolved into its elements, bromine and oxygen. In conclusion, the author remarks that the action of nitric acid on these three classes of salts affords a ready method of distinguishing them from one another.

On the tests for Sulphuric Acid when thrown on the Person. By R. D. THOMSON, M. D.

The object of the author was to discuss the accuracy of the modes of testing sulphuric acid when employed for criminal purposes, and especially when thrown on the person. A case had lately occurred to him in practice, and which was brought before the last sessions of the Central Criminal Court, which proved that the mode of determining the presence of *free* acid by *mere* testing was by no means satisfactory. A woman in a fit of rage threw a quantity of oil of vitriol at the face of a cab-master in the neighbourhood of Euston Square, and before the unfortunate sufferer could wash off the acid two minutes had expired; the consequence was, loss of vision in the eye. The author stated, that having attentively considered this case, and made a series of experiments on the eyes of dead animals, he had discovered that this kind of blindness was perfectly curable, and he had accordingly proposed an operation for this purpose in a paper read at the Medical Section. But besides having his face injured, the hat of the man was dis-

coloured also by the acid. This article of dress was sent to the author, to determine the nature of the agent in this work of destruction. The result of this experiment was, that the injured hat, as well as an uninjured one, contained sulphuric acid, as tested by nitrate of barytes, and a solution of the soluble matter of both states of this article of dress afforded an acid reaction. It was therefore necessary to adopt some method which would afford a discriminatory test between the free and combined acid; the usual mode, viz. by boiling with carbonate of lead, and concluding, if any insoluble sulphate of lead was formed, that the acid existed in a free state, was found to be totally fallacious, because carbonate of lead decomposes sulphate of soda, contrary to the opinion stated in works of medical jurisprudence. Besides, it was shown that many of the usually so called neutral sulphates exhibit, in reality, an acid reaction upon test-paper, as in the instances generally of sulphates of potash, iron, soda, barytes, and also in the cases of alum, &c.; and hence the excess of acid attached to these salts would be apt to act as free acid upon the barytes test. The author, therefore, concludes, that the only demonstrative proof which chemistry affords is a quantitative analysis. Thus he found the entire hat to contain 356 per cent. of sulphuric acid, probably in the state of alum or copperas, and the injured hat 1.379 per cent.; or, in other words, the hat had received by the injury 1.023 per cent. of free sulphuric acid. Here there was afforded clear evidence of the nature of the agent employed to effect the injurious object, which could not have been conclusive if the matter examined had only amounted to a drop or stain. The author directed attention to a point connected with sulphuric acid in a medico-legal point of view, viz. that the oil of vitriol of commerce always contains, in this country, nitric acid, in addition to various other impurities. Barruel has stated, that sulphuric acid is capable of dissolving platinum. The author has not been able to satisfy himself that it dissolves any sensible quantity of gold-leaf. Barruel attributes the property, which he states it to possess, of dissolving platinum, to the sulphuric acid assuming the function of muriatic acid. But the author is not aware of any experiment which would authorize this conclusion. He is rather inclined to attribute the action, if such an occurrence takes place, to the muriatic acid which is present in all the oil of vitriol prepared from sulphur that he has examined. It is given out in sensible quantities when a solid oil, such as cocoa nut oil, is acted on by sulphuric acid. This he ascertained several years ago, when examining some Indian oils, and Dr. Kane has since corroborated the fact of the existence of muriatic acid in oil of vitriol, although the author has not been able to observe the solution of any sensible quantity of gold-leaf by the action of oil of vitriol *per se*; yet if a few drops of muriatic acid be added, the action becomes very powerful, and by the application of heat platinum also is dissolved. These facts, therefore, prove that whenever we have oil of vitriol we may expect also nitric acid. The author added, that he knew

of no certain mode of detecting the presence of nitric acid save by the property which it possessed of dissolving gold and platinum by the addition of muriatic acid. Pure morphia has no action upon nitric acid. It is the resin which generally accompanies that alkalioid which produces the characteristic yellow colour. But the author found that preparations of opium in which the resin was excluded, afforded no colour when nitric acid was added. From an examination of numerous cases of poisoning by opium which had appeared before the Middlesex coroners, he had come to the conclusion that the resin-of-opium test for nitric acid, afforded only an auxiliary method of arriving at the truth, as its characters were frequently usurped by other organic substances.

On the Resin of Sarcocolla. By Professor JOHNSTON.

The resin of sarcocolla of commerce is separated by water into three portions: 1. A gum (A) which does not dissolve in water or alcohol, but which is in a great measure washed out by means of the former solvent. 2. A portion (B) insoluble in water, but soluble in alcohol, which is of a resinous aspect, and is represented by $C_{40}H_{32}O_{14}$. The hydrate is $C_{40}H_{32}O_{14} + 3HO$ when dried at 60° .

This portion B, is separated (decomposed?) by bases into two or more organic compounds, the alcoholic solution giving with neutral acetate of lead a salt containing an organic constituent represented by $C_{40}H_{25}O_{16}$. Ammonia throws down from the mixed solutions a

second salt of lead, the constitution of the organic constituent in which has not yet been determined. 3. The portion taken up by water from the crude sarcocolla, when evaporated to dryness, is separated by alcohol or ether into a soluble (C) and an insoluble portion (D). 4. The soluble portion C dried at 212° , gave discordant results approaching to $C_{40}H_{32}O_{15}$, but when treated with bases

gave salts containing organic constituents of a different constitution. A neutral acetate of lead throws down a salt represented by $P6O + C_{40}H_{28}O_{15}$, and the subsequent addition of the neutral triacetate

acetate a salt represented by $2P6O - C_{40}H_{32}O_{16}$.

5. The portion D, insoluble in alcohol, but soluble in water, consists of a gum and of a substance which is precipitated by neutral acetate of lead in curdy flocks. The investigation is still in progress, and the results are to be considered as open to correction.

On Resins. By Professor JOHNSON.

In this paper the author drew attention to the following facts, apparently established by a table of analytical results, which he exhibited, and had printed:—1. That the resins differ from each

other in the quantity of oxygen they contain. 2. That those in which the atoms of oxygen is the same, the hydrogen may vary, and that this is another cause of difference in the properties of the resins. 3. That in all the resins hitherto carefully analyzed, the number of atoms of carbon is constant. 4. That the resins, as a natural family, may be represented by a general formula containing two variables. 5. That the known resins divide themselves into two groups, possessing unlike chemical and physical properties. That of one of these groups, colophony, may be considered as the type, and that it is represented by $C_{40}H_{32}O_y$; that gamboge, or dragon's blood, may be considered as the type of the other group, which is represented by $C_{40}H_{24}O_y$.

On a New Salt obtained from Iodine and Caustic Soda. By Prof. FRED. PENNY.

While examining the action of iodine on carbonate of soda, a salt was obtained, which crystallized in regular six-sided prisms, and which gave by analysis sodium, iodine, and oxygen, in proportions not corresponding to any known compound of these elements. The same salt was also prepared by saturating a solution of caustic soda with iodine, and allowing the solution to evaporate spontaneously. At first, this salt was thought to be the same as that described by Mitscherlich in his elements of Chemistry, and to which he gives the following composition $NaI \cdot \frac{1}{5} NaO, IO \cdot \frac{1}{20} H O$;

but the analysis gave very different results. Professor Penny gives the following characters of this salt:—It is white and inodorous, has a sharp, saline taste, crystallizes in short six-sided prisms, is soluble in cold and hot water, and is decomposed by alcohol into iodate of soda and iodide of sodium. It effloresces by exposure to the air, and is very readily decomposed by heat; water in abundance is first evolved, and then oxygen with a trace of iodine. Its solution is perfectly neutral to test papers, gives a pale lemon yellow precipitate with acetate of lead, yellowish white with nitrate of silver, and a fine bright yellow with pernitrate of mercury. It is not affected by solution of starch, but instantly decomposed with the precipitation of iodine by nitric, sulphuric, acetic, and hydrochloric acids. The latter acid in excess converts it wholly into chloride of potassium. He detailed a remarkable circumstance attending the formation of this salt from iodine and caustic soda. When the solution is evaporated spontaneously, long prismatic crystals of iodate of soda deposit; but as the evaporation continues, these crystals are re-dissolved, and are replaced by those of the new salt. In one experiment this change was very striking. The solution on Saturday night had deposited an abundance of fine crystals of iodate of soda, but on Monday all these had disappeared, and a crop of the new salt had crystallized. The

prior deposition of iodate of soda generally occurs in the preparation of this salt; and from other experiments of the author, it seems necessary that there should be excess of iodide of sodium present in the solution, and that the solution should be strong, in order that the salt may form. When this salt is dissolved in water, and the solution evaporated spontaneously, crystals of iodate of soda deposit, but very few of the new salt will form. The salt may also be procured by pouring a saturated solution of iodide of sodium on crystals of iodate of soda, and setting them aside for some days. The crystals will be dissolved and be replaced by crystals of the new salt. Prof. Penny then gave the details of his analysis of this salt, and the following formula, as agreeing best with his results:— $\text{Na I O}_5 - \frac{1}{2} \cdot 38 \text{ H O}$; or regarding it as a compound of iodate and iodide, it may be thus represented:— $3 \text{ Na I} - \frac{1}{2} 2 \text{ Na O I O}_5 - \frac{1}{2} \cdot 38 \text{ H O}$. According to this view, it is the sesqui-iodide of iodate of soda.

On the Mode of detecting Minute Portions of Arsenic.

By Dr. Clark, of Aberdeen.

This mode had been applied by the author to the detection of arsenic in commercial specimens of the metals tin and zinc. Grain tin, made in Cornwall, contains arsenic, which seems to be the occasion of the peculiar smell of the hydrogen evolved from that metal by the action of acids. All the specimens of commercial zinc that the author had happened to try were found to contain arsenic. Pure muriatic acid, diluted with distilled acid, is poured upon the metal, and the hydrogen evolved is passed first through a solution of nitrate of lead, and next through a solution of nitrate of silver. Nitrate of lead seems not acted upon by arseniuretted hydrogen,—at least, when in very small proportion; but were any sulphur present in the metal, sulphuretted hydrogen would be evolved in consequence, and the solution of nitrate of lead would be blackened, which, however, the author did not observe ever to occur. But nitrate of silver seems immediately to be acted upon by most minute portions of arseniuretted hydrogen. A bluish black precipitate is formed, which, to judge from a qualitative analysis, appears to be an arseniuret of silver. This bluish black precipitate may be collected with remarkable facility, from its falling readily from the solution, which it leaves perfectly clear. Heated in a small tube, so that the matter heated comes into contact with the air, the bluish black precipitate evolves arsenious acid, which, by the liquid tests, may be further satisfactorily recognized. Antimony produces a similar precipitate, so that the mere appearance of the precipitate is not enough, without the production and recognition, by the usual methods, of the arsenious acid. By a few evident

modifications, this method may be applied to medico-legal investigations.

Dr. R. D. Thomson had found that the electrical method of Mr. E. Davy was inapplicable, in consequence of the deposition of a black matter from the zinc, which he had considered to be bitumen. Dr. Clark has, however, proved it to be arsenic.

On a New Mode of estimating Nitrogen in Organic Analysis. By
PROFESSOR BUNSEN.

The qualitative methods at present employed for the analysis of azotized bodies were shown to be defective, for it is impossible to employ these processes when the nitrogen and the carbon are in small proportion to each other. Prof. Bunsen's process consists in introducing the substance to be analyzed, after having mixed it with oxide of copper, into a glass tube. A few slips of metallic copper are then added, and the tube is fixed to Dobereiner's apparatus for producing hydrogen. This gas is conducted through it until all the atmospheric air is expelled, giving the tube a rotatory motion at the same time, in order to dislodge any air which might be retained between the particles of the oxide of copper. The tube is now hermetically sealed, and introduced into an iron vessel filled with gypsum. The gypsum must be still moist when the tube is introduced, in order that it may be firmly wedged. Thus prepared, it is introduced into the common oven used for organic analysis, and surrounded with red-hot coals. If the tube be of strong green glass it never bursts. When the combustion is completed, the tube is placed below a graduated glass receiver standing over mercury, and the point cut off. The gas which had a pressure of several atmospheres now rushes into the jar. The carbonic acid is absorbed by a ball of hydrated potash, which is introduced into it, and the remaining gas must be nitrogen, for all the hydrogen must have been converted into water by the oxygen of the oxide of copper. The results obtained by this method agree with theory to the second and often to the third decimal place.

MISCELLANEOUS ARTICLES.

On the Cultivation and Growth of Electrotypes.

Without entering into the merits or demerits of the various modes of proceeding, which have been placed before the public, in the process of forming electrotype, I cannot help thinking that there are some theoretic points of very great importance, which remain

to the present moment, unexplained ; indeed, nearly untouched. But as all the processes of art are based on unvarying theoretical principles, and must consequently be prosecuted with much greater facility, and with better success, when guided by theoretical laws, than under other circumstances ; a brief view of the theoretical principles in the process of forming electrotypes may possibly be interesting to many readers of the "Annals."

Whenever an electric current from a voltaic battery is made to traverse an aqueous solution of a metallic salt, sulphate of copper, for instance, a decomposition of the solution is accomplished, and the liberated particles of the metal assemble at the negative pole. And the oxygen and acid matter assemble at the positive pole ; and the terminal negative plate in the solution, has its surface, next to the positive plate, soon covered with a coating of copper. If instead of having two plates only in the solution, there were several, perfectly unconnected with each other, as is shown in fig. 4, plate vi, every plate would become electro-polar, having a positive and a negative surface, as indicated by the letters p n, p n, &c. The positive side of each plate would become oxidized, and the negative side would receive copper from the liquid : and the deposition of copper on the negative side of each plate would form a new compact plate of copper. And if any engraving were on the negative side of any of these plates, or on all of them, the new plates, (the electrotypes) would be complete pictures in relief, of the original engravings. When a single pair of metals is used, and an engraved copper plate is one of them, and a piece of zinc the other, the deposition of copper, from the solution in which they are placed, will be on the engraved copper plate. It was in this way that the electrotype was formed, from which the print accompanying this number was taken.

A wire was soldered to the back side of the engraved plate, and another wire to a similar piece of zinc. The former, with its face upwards, was placed in a solution of sulphate of copper, and the latter in water in a porous paper tray above it. The two wires were tied together by a thin copper wire, which formed the voltaic circuit. The liquids were changed every 24 hours. In five days the first crop was removed from the engraved plate. This first crop was then furnished with a wire and made to assume one side of a new voltaic pair, with a new zinc for the other metal. And by placing this new voltaic pair in similar liquids, and in the same manner, as in the first process ; a *second crop* of electrotype was formed on the face of the first one. This second crop, of course, is a fac simile of the original engraved plate ; and in six days became 4 ounces heavier than it. We have other plates growing at the Victoria Gallery, from which prints will be taken, and presented to the readers of the "Annals."

Description of the Dial Plate of Professor Wheatstone's Electro-Magnetic Telegraph.

Figure 6 of plate vii., is a front view of the dial plate of the telegraph, on which are placed twenty-five letters of the alphabet, and five indexes. The indexes are placed in a horizontal line on the letters L, M, N, O, P, one on each : and by means of magnetic needles placed on the same axis, behind the dial plate, and those needles placed within spiral conductors in the usual way, the indices can be deflected either towards the right or the left, according to the direction of the electric current which traverses the conductor. When only one needle is deflected, it indicates the letter on which it is placed. All the other letters are indicated by being pointed at by two needles. The letter F, is pointed at by two needles in the figure, and is consequently the letter indicated by the telegraph. Other letters on the dial plate are pointed at in a similar manner.

On the remarkable diffusion of Coralline Animalcules from the use of Chalk in the arts of life, as observed by Ehrenberg.—An examination of the finest powdered sorts of chalk which are used in trade, has afforded Prof. Ehrenberg the following result : that even in this finest condition, not merely the inorganic part of the chalk is become separated, but that it remains mixed with a great number of well-preserved forms of the minute shells of coral animalcules. As powdered chalk is used for paper hangings, Prof. Ehrenberg also examined these, as well as the walls of his chamber which were simply washed with lime, and even a kind of glazed vellum paper called visiting cards, and obtained the very visible result—demonstrating the minuteness of division of independent organic life ; that those walls and paper-hangings, and so, doubtless, all similar walls of rooms, houses, and churches, and even glazed visiting cards prepared in the above mentioned manner, (of which cards, however, many are made with pure white lead without any addition of chalk,) present, when magnified three hundred diameters, and penetrated with Canada balsam, a delicate mosaic of elegant coralline animalcules, invisible to the naked eye, but, if sufficiently magnified, more beautiful than any painting that covers them.—*Annals of Natural History*, p. 286, No. 24, for December, 1839.

Auroral belt of May 29, 1840.—About 9h. 20m. P. M. of Friday, May 29, 1840, a luminous belt, spanning the heavens from east to west, was seen by several persons in this city. When fully formed, about 9h. 22m., its width was from 3° to 5° , being widest and most luminous on the western portion ; its altitude, at the highest part, about 85° above the southern horizon. Its light was similar and equal to that of ordinary auroral streamers. The extremities of the belt were 10° or 20° above the horizon, but their position was not

particularly noted, and may have varied 10° or more from the E. and W. points. The northern edge of the belt was well defined; the southern was not very distinct. The belt slowly drifted southward, at the rate of about a degree per minute. At 9h. 30m., at which time the belt was brightest and most perfect, its northern edge was projected on Arcturus. Just before the belt reached this star, there was a slight bending, concave to the north, in that part of the belt which lay not far east of the meridian. This occurred near that region of the heavens in which (at this town) an auroral corona is manifested. The belt soon began to fade, and by 9h. 45m. was nearly extinct, but for ten minutes longer, a small remnant of it was visible in the southwest, which, just before it disappeared, passed to the south of Regulus. The summit of the belt was, at vanishing, about 10° south of Arcturus. This belt was apparently constituted in part of beams obliquely transverse to its length, but this character was on this occasion less conspicuous than has commonly been noticed in other cases. During the whole time the sky was obscured by haziness and partially by clouds. There was some auroral light about the northern horizon, but it had no visible connection with the belt. Soon after 10h. this light increased exceedingly; numerous streamers rose to the altitude of 50° , and auroral waves flashed up nearly or quite to the coronal point.

This auroral belt was seen at New York city, and doubtless at many other places. If observations upon the position of the edge of the belt at given times were made at any considerable distance north or south of New Haven, we might have the means of finding approximately its height above the earth. If any such observations were made, it is to be hoped that they will be given to the public.

E. C. HERRICK.

New Haven, Connecticut.

Lectures on Electricity, Magnetism, &c.

LECTURE II.

Having, in the first lecture, given specimens of the electric, the magnetic, and the calorific classes of phenomena, I will now proceed to offer to your notice a few other preliminaries which will be necessary to be understood before we can enter very far into the study of electricity. In the first place, then, I must present to your notice a very well established fact respecting a property of atmospheric air, which is applicable to all the gases, and also to

the electric matter. When the air within the receiver of an air-pump has become attenuated by the action of the pump, it still occupies the whole capacity of the receiver, and does not settle, as water would do, into the lower part of the receiver, so as to occupy that part only. Let us, for example, suppose that the receiver originally contained a quantity of air which we will call 100. If, now, by the action of the pump, 50 parts were to be withdrawn, the receiver would retain the other 50 parts only, or just one-half of the original quantity. But these 50 remaining parts of the air would still occupy the whole capacity of the receiver. Suppose, now, that the pump is again set to work, and that it withdraws from the receiver just one-half of the 50 parts that were left by the first operation; it is easy to understand that since the half of the 50 parts has left the receiver, there can be only 25 parts remaining. But these 25 parts, which are only a quarter of the original quantity, do not subsist in their original dimensions, and so occupy one quarter only of the receiver; nor do they subsist in one-half of the capacity of the receiver, in their dimensions previous to the last operation of the pump; but absolutely fill the whole capacity of the receiver as decidedly as the 100 original parts filled it. And in the same manner the whole capacity of the receiver would be occupied by any remaining portion of air, even after that portion had become too small for the pump to affect it any longer. Now in all these cases, it is obvious that the air has expanded by virtue of some inherent power with which it is naturally endued. This power is usually called *repulsion*: and it is admitted by all philosophers that the particles of air have a natural inherent repulsive force, by means of which they are continually endeavouring to recede from one another. Hence it is that air becomes expansible to an amazing degree, and any portion of it may be made to occupy a space immensely greater than that which it occupies naturally at the surface of the earth.

On the other hand, any portion of the air at the earth's surface may be condensed into a smaller and smaller compass than that which it naturally occupies. If, for instance, an inverted glass tumbler were to be held just over the surface of the water contained in a glass jar, it would contain a certain quantity of air, which would occupy the whole capacity of the tumbler; but if this tumbler with its contained air, were to be pressed down into the water, the air would no longer occupy the whole capacity, but would be compressed into a less space, and a portion of water would enter the lower part of the inverted vessel: and the deeper in the water the confined air was taken, the less space would it occupy. This is a very decisive experiment, and the simplest I can think of for showing the compressibility of air. A small piece of cork may be placed on the surface of the water beneath the tumbler, which will always indicate the height to which the water ascends inside, at different depths, and consequently show the

space occupied by the air. Having now become acquainted with these two facts, the expansibility and the condensibility of air, we learn that air has the quality of being *elastic*. But it must not be forgotten that this *quality of elasticity* which air possesses is a mere consequence of the natural *inherent repulsion* of its particles.

By keeping in view the consequences of the attribute of repulsion which air possesses, whilst contemplating electrical phenomena we shall be enabled to account for a great variety of facts which would otherwise appear inexplicable. The electric matter, or, the electric fluid, as it is more frequently called, is much more highly elastic than common air, and therefore can be condensed and attenuated by employing proper means to a very great extent; but its motions, when in the act of expanding, are performed with such an immense degree of activity that, although several philosophers have attempted to ascertain its velocity, their efforts have hitherto been unavailing.

Besides the quality of elasticity in common with air, and other kinds of gaseous matter, the electric fluid possesses others peculiar to itself. Its activity is superior to that of any other known kind of matter: it enters into the pores of the most compact solids, and is to be found in every kind of tangible matter. It constitutes a portion of the atmosphere, and frequently accumulates to an amazing extent in the clouds, gradually increasing in density, till its elasticity becomes sufficiently great to enable it to burst from its aerial prison in a compact form, and exhibit itself in all the majesty and splendour of lightning.

It is a remarkable fact that the motions of the electric fluid are much more facilitated by some classes of bodies than by others. The metals are considered to facilitate the progress of the electric fluid to a greater extent than any other class of bodies whatever. But the metals themselves, as individual bodies, vary very considerably in the degree of facility which they respectively offer to the motions of the electric fluid: copper offering the greatest facility of any known body, and lead, or iron, perhaps, the least of any of the metals. But it would be impossible, in the present condition of the science, to give a correct table of the various degrees of facility which different bodies offer to the motions of the electric fluid: for although much has been attempted to be done, and much more pretended to have been done, in determining so important a particular in electricity, it is lamentable in the extreme to have to acknowledge that but very little has absolutely been accomplished in this interesting inquiry.

Those bodies which offer comparatively great facilities for the motion of the electric fluid, are usually called *conductors*; and those which offer the least facility, being supposed to present an absolute *resistance* to the motions of the fluid, have been called *non-conductors*. Now, as the terms *conductors* and *non-conductors* of electricity, are well known from their long use, and as I am not disposed to

attempt to supplant by others, any familiar technicalities, such as these, which have been of considerable benefit in the promotion of the science, I can find no objections to place before my readers the following tables of what has been considered *conductors* and *non-conductors* of electricity, which I find in Mr. Singer's excellent "Elements of Electricity":—

CONDUCTORS.

All the known metals,
Well burnt charcoal,
Plumbago,
Concentrated acids,
Powdered charcoal,
Diluted acids and saline fluids,
Metallic ores,
Animal fluids.
Sea water,
Spring water,
River water,
Ice and snow,
Living vegetables,
Flame,
Smoke,
Steam,
Most saline substances,
Rarefied air,
Vapour of alcohol and æther,
Most earths and stones.

NON-CONDUCTORS.

Shell-lac, amber, resins,
Sulphur, wax, jet,
Glass, and all vitrifications; talc.
The diamond, and all transparent
jems,
Raw silk, bleached silk, dyed silk,
Wool, hair feathers,
Dry paper, parchment, & leather
Air, and all dry gases,
Baked wood, dry vegetable sub-
stances,
Porcelain, dry marble,
Some silicious and argillaceous
stones,
Camphor, elastic gum, lycopo-
dium
Native carbonate of barytes,
Dry chalk, lime, phosphorous,
Ice at — 13° of Fahrenheit's
thermometer
Many transparent crystals, when
perfectly dry,
The ashes of animal and vegeta-
ble substances,
Oils, the heaviest appear the best,
Dry metallic oxides.

Mr. Stephen Gray, a pensioner of the Charter House, was the first person to discover the conducting power of metals, and to ascertain the great difference, in this respect, between a metallic wire, and a cord of hemp, or silk. This discovery was made on the 3d of July, 1729, it was perfectly accidental, and occurred from the circumstance of substituting a metallic wire for the suspension of an electrized body, in lieu of a silken cord which had broken. Dr. Priestley, at the suggestion of Dr. Franklin, seems to have been the first philosopher who undertook a series of experiments, for the purposes of ascertaining the different degrees of conducting power possessed by different bodies. Several other philosophers have also paid considerable attention to this subject, though, as I have before stated, little more has been accomplished than the ascertaining of a few general facts: for there still remains much difference in the tables given by different authors. The

following table is taken from Cavallo's "Complete Treatise of Electricity," 2nd edition published in 1782:—

CONDUCTORS.	NON-CONDUCTORS.
Gold,	Glass, and all vitrifications, even those of metals,
Silver,	All precious stones: the most transparent the best,
Copper,	All resins & resinous compounds,
Brass,	Amber,
Iron,	Sulphur,
Tin,	Baked Wood,
Quicksilver,	All bituminous substances,
Lead,	Wax,
Semi-metals,	Silk,
Animal and vegetable charcoal,	Cotton,
The fluids of the human body,	All dry animal substances, as
All fluids, excepting air and oils,	feathers, wool, hair, &c.
The effluvia of flaming bodies,	Paper,
Ice,*	White sugar,
Snow,	Sugar candy,
Most saline substances, the best being metallic salts,	Air,
Soft stoney substances,	Oils,
Smoke,	Calces of metals,
The vapours of hot water,	The ashes of animal and vegetable substances,
Highly attenuated air.	All dry vegetable substances,
	All hard stones, the hardest the best.

Professor Cumming, in his translation of Demouffrand's "Manual of Electro-Dynamics," gives the following table of the conducting powers of metals—

Silver,	Tin,
Copper,	Platina,
Lead,	Palladium,
Gold,	Iron.†
Brass, zinc,	

It would be useless to give any more tables of the conducting and non-conducting powers of different kinds of matter, as there are no two that agree in every particular. For my own part, I am of an opinion that all bodies are conductors more or less, metals being the best class of conductors, and vitrious and resinous substances being about the worst. Much, however, will depend upon the extent of the electric force employed, and much again upon the length of the bodies upon which that force has to operate.

When any body in a state of electrization is supported on a non-

* According to Achard, ice conducts the electric fluid whilst it remains above a certain temperature, but is not a conductor below that temperature.

† In all these tables, those bodies which are first in the list, are considered the best of their kind; and the others take precedence of all those below them.

conductor, as by a glass stem, or suspended by silk or other non-conductor, it is said to be *insulated*. There are many other technicalities which I shall have to notice as I proceed, but it will not be necessary in this place to introduce any more than those already mentioned.

The motions of light bodies by the action of sealing-wax, glass, &c., already noticed in the last lecture, are phenomena of a high interesting character, and are a portion of those which must necessarily be regarded as of an elementary character, independently of a knowledge of which no plausible hypothesis of electricity could possibly be formed: on which account it will be necessary to recur to them again, and to point out other experiments from which similar results may be obtained. But it must not be expected that because the results, by various modes of experimenting, are similar or of precisely the same character, that they should be of precisely the same extent, or degree of power. The light emanating from two burning candles of different dimensions, may be, and generally is, of precisely the same character, but the *intensity* of the light, from the two sources, may be very different: or, we may say, the *quantity* of light proceeding from one of the candles is very different to the *quantity* of light proceeding from the other. If similar reasoning be applied to the display of electrical phenomena, we may easily understand that, notwithstanding the identity of the *character* of the motions produceable from different sources of electric action, the quantity or intensity of those motions may be very different. And as some sources are sufficiently vigorous to put into motion bodies of a considerable magnitude, and others so exceedingly feeble as to require the employment of the most delicate apparatus for their detection, it will be necessary, before proceeding to other experiments, to describe such instruments or pieces of apparatus as may be wanted for carrying on those experiments with which we ought to be made familiar as soon as possible.

The instruments which are usually employed for the detection of feeble electric action, are called *electroscopes*, of which we have several forms. The simplest electroscope, and one which may be frequently employed, is merely a single fibre of flax, silk, or any other such flexible article as will bend to slight electric forces. The fibre may be supported in any manner you please, so that it be permitted to hang freely in a vertical direction. Fig. 4, plate vii., is an electroscope of this kind, where the fibre *f* is supported by, and hangs freely from, the wooden stem *s*. Having rubbed a stick of sealing wax against the sleeve of your coat, present it to the lower end of the suspended fibre, and you will see that it bends towards the sealing wax; and if you bring them sufficiently near to each other, the fibre will adhere to the wax for some considerable time. In this experiment you have an electric *attraction* exhibited as decidedly as by the motions of the pieces of paper in the

former experiments: but if the fibre be not very dry and warm, you have not that jumping to and fro, as with the pieces of paper, for the fibre of the electroscope clings to the wax without leaving it, till the electric force is so far exhausted as to be no longer able to hold the fibre to the surface of the wax. The action will, in many instances, continue a long time, and by paying attention you may observe the fibre to change places on the surface of the wax, and this very frequently, if you accommodate the wax the motions of the fibre, by moving the former so as to facilitate the motions of the latter. If, however, the fibre be very dry and somewhat warm, it will sometimes recede from the surface of the wax, in the same manner as the pieces of paper, and will lean towards the stem *s*, if very near to it, and even strike against it, and after remaining attached to it a short time, will again return to the wax: and repeat these motions several times, till the electric force is too far exhausted to produce them any longer.

I will not detain you, in this place, with an explanation of the cause of the electroscope fibre continuing to be attached to the surface of the excited wax under some circumstances, and not under others; because I am desirous of first making you acquainted with the structure and method of using another simple electroscope, which will exhibit the principles upon which they are founded in better perfection, than by that made of a single fibre.

Fig. 5, plate vii, will represent the form of a very simple electroscope which may be used to great advantage in some electric inquiries. It consists of a glass stem fixed in a wooden foot, and a projecting horizontal brass wire arm, terminated with a small brass ball. When the foot of this instrument is made of nicely turned and polished mahogany, and the brass arm and its ball well polished and lacquered, the instrument assumes a very pretty appearance. Over the farther end of the horizontal arm is hung a flaxen fibre, to each end of which is attached a very small ball of the pith of the elder. As the glass stem of this instrument is a non-conductor, it is incapable of carrying off any of the electric action of the horizontal arm, or of the fibres and their balls; and as the atmospheric air is also a non-conductor, all that part of the instrument which is supported by the glass stem is insulated. As, however, glass has a great tendency to collect moisture on its surface, the stem of this instrument must be kept warm, and occasionally wiped with dry cloth to preserve insulation as far as possible. If the surface of the glass be covered with a good coating of lac-varnish, the insulation may be maintained for a long time without much trouble.

Let us now again excite the stick of sealing wax, and afterwards present it to the upper side of the horizontal arm of the electroscope, fig. 5. The pith balls will diverge from each other, as represented in the figure, before the wax comes into contact with the metallic arm. But if the wax be withdrawn without touching the metallic arm, the balls will again collapse, and show no electric action.

Excite the wax again, and bring it into contact with the arm of the electroscope, drawing its surface over the arm. The pith balls will diverge as before, and will remain divergent, even when the wax is withdrawn. And if the room, in which the experiment is made, be dry and warm, and the air perfectly still, the balls will remain divergent for a long time, even several hours. But in all cases the divergency will gradually lessen from the first moment that the excited sealing wax is withdrawn from the electroscope, and, eventually, the divergency entirely disappears. Precisely the same kind of phenomena are displayed by the action of any excited body whatever, provided its electric forces be sufficiently powerful.

Hence you may employ excited glass, amber, sulphur, paper, &c., in your experiments with this instrument, and the pith balls will diverge with each excited body. Dry writing paper rubbed with indian-rubber, becomes highly electric; and so does coarse brown paper when drawn quickly between the coat sleeve and a woollen table cloth; or between the coat sleeve and the trousers. When the paper is made pretty warm before the friction is given to it, and the knuckle presented afterwards to its surface, a crackling noise will be heard, and sometimes sparks will be seen between the knuckle and the paper. This experiment answers best during frosty weather. Similar phenomena may be produced by stroking the back of a cat. Puss often becomes uneasy by this treatment, and the hairs of her back and tail brush out in a very strange manner.

I must now bring forward an experiment the results of which are something different to any I have yet offered to your notice. Excite the sealing wax as before and draw it over the arm of the electroscope fig. 5, and when taken away the balls will remain divergent. Again excite the wax, and again make it approach the arm, on the upper side, but without touching it, you will observe the balls separated further than before, but as you withdraw the wax again, the balls will fall to their former position. The balls may be made to separate further from, or approach nearer to, each other, for several times, by alternately advancing the excited wax to the arm of the instrument, and withdrawing it from it. If, after the balls have been divergent by the first application of the wax, the latter be again excited and then presented towards the balls, you will observe them to recede from it: and with a little practise, you may deflect the balls from the wax in any direction you please.

ERRATA.—Page 198, in the heading of article XXV. for “Van Kobell,” read “Von Kobell: and the same in the heading at the top of each left hand page of that article. Line 6 of that article, for “into” read “on to.”—Page 199, line 2, from bottom of page, for “Demerara,” read “Damara.” Same page, first line in the note, for “end” read “and.” In the article “Electrotype,” in pages 237 and 238, all that part which is below Mr. Cartwright’s letter in page 238, is to be read after the word “engraving,” at the end of the third paragraph, page 237.

